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PRACTICAL SCIENCE[1]

MEN who spend their lives in universities are apt to develop certain unfortunate peculiarities. These peculiarities may not make them less happy, or less useful to their professional students, but they diminish the appreciation of the community at large. In the life of an instructor or investigator of university rank there is a peculiar kind of isolation that is bound to react.

It is partly the isolation of a subject, which is more or less segregated from general human interests, at least in the aspects of it the university man is cultivating. As a consequence, he feels that his world is quite apart from that one in which the majority of men are living. He is conscious of an interest distinct from their interests, which seem therefore relatively trivial. This sense of intellectual aloofness does not result in a feeling of loneliness, but rather in a feeling of superiority, unconscious in many cases, but often naïvely expressed.

It is also the isolation of authority, which comes from mastery of a subject and from association with students who recognize this mastery. To speak with authority in intellectual matters, to give the deciding word, to meet a constant succession of inferiors, is apt to affect any man's brain. Either he becomes dogmatic in expression, or he must hold himself in check with an effort. It is the same reaction that was observed in the case of the clergy, when acknowledged authority in position

[1] Address at the winter convocation, 1910, of the University of Chicago.

resulted in an assumption of authority in belief.

The larger the university, the more intense does this sense of the isolation of superiority and of authority become, for it is stimulated by association with its own kind. There is much honest effort to break down this barrier between the scholars who represent universities and the great host of men who represent the community. These men are not so isolated, but they are just as dogmatic in their own way, and they are immensely influential. Even when the two groups mingle, the scholar is often only a man of incidental interest, who possesses much curious information about many useless things. And the scholar usually enjoys being drawn out and made to display his curiosities, for it has the familiar flavor of the classroom, with its intellectually inferior students.

Of course such contact between scholar and community is not the effective one, for it is merely that of audience and entertainer. Here are two groups of men, both powerfully equipped, who should be mutually stimulating in all that makes for progress. Mutual stimulation can follow only after mutual understanding. It is not for me to explain the community to the scholar, but rather to explain the scholar to the community. Even this subject is far too large, for scholarship has many phases, all the way from artistic appreciation to scientific synthesis. I shall try to explain in outline only the scientific aspect of scholarship, and its significance to the community.

It is evident that the public is somewhat interested in scientific research. The most available index of the present interest is furnished probably by the newspapers and magazines, which try either to respond to the desires of their readers, or to cultivate desires. Even a cursory examination of the material they furnish, which may be said to deal with research, shows that it is scanty in amount, sensational in form and usually wide of the mark. The fact that it is scanty in amount is a cause for congratulation, if it must involve the two other features. The sensational form is a concession to what is conceived to be public taste; and while to a scientific man this form seems to exhibit the worst possible taste, the serious objection is that to secure the form truth is usually sacrificed. Some of the results of this kind of information are as follows:

Men engaged in research are looked upon in general as inoffensive but curious and useless members of the social order. If an investigator touches now and then upon something that the public regards as useful, he is singled out as a glaring exception. If an investigation lends itself to announcement in an exceedingly sensational form, as if it were uncovering deep mysteries, the investigator becomes a "wizard," and his lightest utterance is treated as an oracle. The result is that if the intelligent reading public were asked to recite the distinguished names in science, they would name perhaps one or two real investigators unfortunate enough to be in the public eye, several "wizards," and still more charlatans. The great body of real investigators would be known only to their colleagues, thankful that they were not included in any public hall of fame. And yet the public is not to be blamed, for it is giving its best information; and the fact that it has even such information indicates an interest that would be wiser were it better directed. This better direction is dammed up behind a wall of professional pride, which makes an investigator look askance at any colleague who has broken through it. The intelligent public is certainly interested, but it is just as certainly

not intelligently interested. I wish to analyze the situation briefly.

There is a conventional application of the term science, which I will use for convenience. Thus applied, there has arisen a classification of science into two phases, called pure science and applied science. This distinction is one that not only exists in the public mind, but it is also reinforced by published statements from colleges and universities. An attempt to define these two kinds of science reveals the fact that the distinction is a general impression rather than a clear statement. A general impression is usually sufficient for the public, but it ought not to be sufficient for the universities.

If the impression be analyzed, it seems that pure science is of no material service to mankind; and that applied science has to do with the mechanism of our civilization. The distinction, therefore, is based upon material output. In other words, pure science only knows things, while applied science knows how to do things. This impression, rather than distinction, has been unfortunate in several ways.

The public, as represented by the modern American community, believes in doing things; and therefore to them pure science seems useless, and its devotees appear as ornamental rather than as vital members of human society, to be admired rather than used. The reaction of this sentiment upon opportunities for the cultivation of pure science is obvious.

On the other hand, the universities, as represented by their investigators, believe in knowing things; and therefore to them applied science seems to be a waste of investigative energy, and its devotees appear to be unscientific, very useful, but not to be acknowledged as belonging to the scientific cult.

The reaction of this sentiment sometimes has been to avoid the investigation of problems that have an obvious practical application, and to justify Lowell's definition of a university as "a place where nothing useful is taught."

In this atmosphere of mutual misunderstanding the public and the universities have continued to exist and to make progress, all the time acknowledging their interdependence by mutual service.

In recent years, however, a new spirit is taking possession of the public and it has invaded the universities. In fact, so conspicuous have the universities become in the movement that they seem to be the leaders; certainly they furnish the trained leaders. The new spirit that is beginning to dominate increasingly is the spirit of mutual service. It is called by a variety of names, dependent upon the group that proclaims it; it is narrow or broad in its application, dependent upon the moral and intellectual equipment of its promoters; but it is the same enduring idea.

The university is no longer conceived of as a scholastic cloister, a refuge for the intellectually impractical; but as an organization whose mission is to serve society in the largest possible way. Furthermore, this service is conceived of not merely as the indirect contribution of trained minds, a contribution of inestimable value, as we believe; but also as the direct contribution of assistance in solving the problems that confront community life.

This new animating spirit is so attractive and inspiring, appealing to what seem to be our best impulses, that it threatens to become a real danger not only to universities, but to the whole scheme of education down to the primary school. The reaction is natural, and therefore inevitable; but its demands must be recognized as representing the primary and extreme recoil stage of a new motive. The new motive must not

eliminate all the old motives, but must adjust itself efficiently among them. For example, there is abroad an increasingly insistent demand that in the primary and secondary schools all instruction in pure science shall be discarded and various forms of applied science substituted, the imaginary distinction being that which has been indicated. The same pressure is being felt in the college, not to the extent of substitution, but to the extent of adding impossible courses and weakening existing ones. My present thesis, however, is interested chiefly in the fact that the same pressure has begun to be applied to the research work at universities. This pressure is applied not only by public demand, which voices the supporting constituency of most universities, especially of the middle west; but also by the extensive scientific work of state and federal governments, in which for the most part the immediate practical aspect must dominate. The more recent developments at our state universities are impressive illustrations of this pressure; and as a result, in such universities scientific research, in connection with problems that do not seem to be related at present to the welfare of the community, is living in a depressing atmosphere.

It is time for the public and for the managers of universities to understand the real relation that exists between what they have been pleased to call pure science and applied science. I can not hope to make a statement that will appeal to all concerned, but it may serve some useful purpose.

As an introductory illustration, there may be outlined the usual steps that science has taken in the material service of mankind. An investigator, stimulated only by what has been called "the delirious but divine desire to know," is attracted by a problem. No thought of its usefulness in a material way is in his mind; he wishes simply to make a contribution to knowledge. No one can appreciate the labor, the patience, the intellectual equipment involved in such work unless he has undertaken it himself. The investigator succeeds in solving his problem, and is satisfied. Later, perhaps many years later, some other scientific man discovers that the results of the former may be used to revolutionize some process of manufacture, some method of transportation or communication, some empirical formula of agriculture, some practise in medicine or surgery. The application is made and the world applauds; but the applause is chiefly for the second man, the practical man. Any analysis of the situation, however, shows that to the practical result both men contributed, and in that sense both men, the first no less than the second, were of immense material service. The ratio that exists between scientific men of the first type and those of the second is not known, but there is very great disparity.

Another illustration is needed as a corollary. In this case an investigator, stimulated by the desire to serve the community, is attracted by a problem. He also wishes to make a contribution to knowledge. He succeeds in solving his problem, perhaps makes his own application, and is satisfied. Later, some other scientific man discovers that the results of the former may be used to revolutionize certain fundamental conceptions of science. His statement is made and the scientific world applauds; and this time also the applause is chiefly for the second man, the pure scientist. The analysis of this case shows, however, that to the scientific result both men contributed; and that both men were of large scientific service.

A third illustration is needed to complete the real historical picture of progress

in scientific knowledge and in its material applications. A practical man, not trained as an investigator, faces the problem of obtaining some new and useful result. His only method is to apply empirically certain formulæ that have been developed by science, but with ingenuity and patience he succeeds, although he is not able to analyze his results. And yet, his procedure reveals to a trained investigator a method or certain data that lead to a scientific synthesis of the first order.

With such illustrations taken to represent the actual historical situation, what may be some of the conclusions?

It is evident that responsibility for the material results of science is to be shared by those engaged in pure science, those engaged in applied science and those not trained in science at all. The only distinction is not in the result, therefore, but in the intent. As one of my colleagues has aptly said, the difference between pure science and applied science, in their practical aspects, resolves itself into the difference between murder and manslaughter; it lies in the intention. So long as the world gets the results of science, it is not likely to trouble itself about the intention. In every end result of science that reaches the public, there is an inextricable tangle of contributions. Between the source of energy and the point of application, there may be much machinery, and perhaps none of it can be eliminated from the final estimate of values. And yet, the public is in danger of gazing at the practical electric light and forgetting the impractical power house; and schools are being asked to turn on the electric light and to shut off the power house.

Another conclusion is that all application must have something to apply, and that application alone would presently result in sterility. There must be perennial contributions to knowledge, with or without immediately useful intent, that application may possess a wide and fertile field for cultivation. It is just here that the menace to education is evident. When education in science becomes a series of prescriptions, to be followed without understanding and without perspective, it will train apprentices rather than intelligent thinkers. Of course there is a place for just this kind of training and there are individuals who need it; but the place does not seem to be the schools for general education, and the individuals are evidently not all those who pass through these schools, or even a majority of them.

A third conclusion is that there is nothing inherent in useful problems that would compel their avoidance by an investigator who wishes to contribute to knowledge. While such an investigator should never be handicapped by the utilitarian motive, at the same time he should never be perversely non-utilitarian. I feel free to make this statement, for perhaps no field, within the confines of my own general subject, seems to be more non-utilitarian than the special one I have chosen to cultivate. There is no reason why a university, especially one dominated by research, should not include among its investigations some that are of immediate concern to the public welfare.

A final conclusion may be that all science is one; that pure science is often immensely practical; that applied science is often very pure science; and that between the two there is no dividing line. They are like the end members of a long and intergrading series; very distinct in their isolated and extreme expression, but completely connected. If distinction must be expressed in terms where no sharp distinction exists, what seems to me to be a happy suggestion, made by one of my colleagues,

is the distinction expressed by the terms fundamental and superficial. They are terms of comparison and admit of every intergrade. In general, a university devoted to research should be interested in the fundamental things of science, the larger truths, that increase the general perspective of knowledge and may underlie the possibilities of material progress in many directions. On the other hand, the immediate material needs of the community are to be met by the superficial things of science, the external touch of more fundamental things. The series may move in either direction, but its end members must always hold the same relative positions. The first stimulus may be our need, and a superficial science meets it, but in so doing it may put us on the trail that leads to the fundamentals of science. On the other hand, the fundamentals may be gripped first, and only later find some superficial expression. The series is often attacked first in some intermediate region, and probably most of the research in pure science may be so placed; that is, it is relatively fundamental; but it is also relatively superficial. The real progress of science is always from the superficial toward the fundamental; and the more fundamental are our results, the more extensive may be their superficial expression. In short, my subject, "practical science," is no subject at all, if it implies a special kind of science, for all science is practical.

I can not leave science in the position of working on the chance that some of its results some day may be found to be of material service to mankind. I have been speaking the language of those who measure usefulness in terms of its market price, and even at that low level the results of science easily control the market. Perhaps there are some who think that this is the only level at which the usefulness of science is conspicuous; for it is often thought of as the Pullman car of our civilization, and not the passenger; something that contributes to our convenience and comfort, but something quite apart from our intellectual and moral selves.

To my mind, the largest usefulness of science, its contribution of immeasurable value to human welfare, is on the intellectual level. It has developed and is continuing to develop the scientific attitude of mind, an attitude that has literally revolutionized thinking, so that all subjects and all education have become scientific. No more impressive testimony to this wide and revolutionary influence of the scientific spirit could be given than that contained in the numerous memorial volumes of last year in honor of Charles Darwin, for his contribution was not so much the theory of natural selection as the scientific point of view. Perhaps the volume from his own university illustrates this most compactly. It contains papers written by 29 men, easily among the leaders in their respective fields, and representing the widest possible range of universities, and all united in saying that this embodiment of the scientific spirit revolutionized not only zoology and botany and geology and astronomy, but also the study of language, of history, of sociology, of philosophy and of religion. This means that all subjects worthy of study and worthily studied have become scientific. It also means that this same scientific attitude is available for our social problems, immensely more important and vital than our material problems, for they deal with human welfare. Without attempting to analyze in any adequate way what has been called the scientific attitude of mind, or the scientific spirit, I wish to indicate three of its useful characteristics.

1. *It is a spirit of enquiry.*—In our ex-

perience, we encounter a vast body of established belief in reference to all important subjects, such as society, government, education, religion, etc. It is well if our encounter be only objective, for it is generally true, and a more dangerous fact, that we find *ourselves* cherishing a large body of belief, often called hereditary, but of course the result of early association. Nothing seems more evident than that all this established belief that we encounter belongs to two categories: the priceless result of generations of experience, and heirloom rubbish. Toward this whole body of belief the scientific attitude of mind is one of unprejudiced inquiry. So far as the attitude is prejudiced, it is unscientific. This is not the spirit of iconoclasm, but an examination of the foundations of belief. It is evident that this spirit is diametrically opposed to intolerance, and that it can find no common ground with those who affirm confidently that the present organization of society is as good as it can be; that our republic represents the highest possible expression of man in reference to government; that the past has discovered all that is best in education; that the mission of religion is to conserve the past rather than to grow into the future. This is not the spirit of unrest, of discomfort, but the evidence of a mind whose every avenue is open to the approach of truth from every direction. For fear of being misunderstood, I hasten to say that this beneficial result of scientific training does not come to all those who cultivate it, any more than is the Christ-like character developed in all those who profess Christianity. I regret to say that even some who bear great names in science have been as dogmatic as the most rampant theologian. But the dogmatic scientist and theologian are not to be taken as examples of "the peaceable fruits of righteousness," for the general ameliorating influence of religion and of science are none the less apparent. It is not the speech of the conspicuous few that is leavening the lump of human thought, but the quiet work of thousands of teachers.

2. *It is a spirit which demands that a claimed cause shall be demonstrated.*— It is in the laboratory that one first really appreciates how many factors must be taken into the count in considering any result, and what an element of uncertainty an unknown factor introduces. Even when the factors of some simple result are well in hand, and we can combine them with reasonable certainty that the result will appear, we may be entirely wrong in our conclusion as to what in the combination has produced the result. For example, the forms of certain plants were changed at will, by supplying to their surrounding medium various substances. It was easy to obtain definite results, and it was natural to conclude that the chemical structure of these particular substances produced the result, and our prescription was narrowed to certain substances. Later it was discovered that the results are not due to the chemical nature of the substances, but to a physical condition developed by their presence, a condition which may be developed by other substances or by no substances; and so our prescription was much enlarged.

There is a broad application here. In education, we are in danger of slavery to subjects. Having observed that certain ones may be used to produce certain results, we prescribe them as essential to the process, without taking into account the possibility that other subjects may produce similar results. In religion, we are in danger of formulating some specific line of conduct as essential to the result, and of condemning those who do not ad-

here to it. That there may be many lines of approach to a given result, if that result be a general condition, is a hard lesson for mankind to learn.

If it is so difficult to get at the real factors of a simple result in the laboratory, and still more difficult to interpret the significance of factors when found, in what condition must we be in reference to the immensely more complex problems that confront us in social organization, government, education and religion, especially when it is added that the vast majority of those who have offered answers to these problems have had no conception of the difficulties involved in reaching truth? The proper effect of such knowledge is not despair, but an attentive and receptive mind.

The prevailing belief among the untrained is that any result may be explained by some single factor operating as a cause. They seem to have no conception of the fact that the cause of every result is made up of a combination of interacting factors, often in numbers and combinations that are absolutely bewildering to contemplate. An enthusiast discovers some one thing which he regards and perhaps all right-thinking people regard as an evil in society or in government, and straightway this explains for him the whole of our present unhappy condition. This particular tare must be rooted up, and rooted up immediately, without any thought as to the possible destruction of the plants we must cultivate.

This habit of considering only one factor, when perhaps many are involved, indicates a very primitive and untrained condition of mind. It is fortunate when the leaders of public sentiment have gotten hold of one real factor. They may overdo it, and work damage by insisting upon some special form of action on account of it, but so far as it goes it is the truth. It is more apt to be the case, however, that the factor claimed holds no relation whatsoever to the result. This is where political demagoguery gets in its most unrighteous work, and is the soil in which the noxious weeds of destructive socialism, charlatanism and religious cant flourish.

3. *It keeps one close to the facts.*—There seems to be abroad a notion that one may start with a single well-attested fact, and by some logical machinery construct an elaborate system and reach an authentic conclusion, much as the world has imagined that Cuvier could do if a single bone were furnished him. The result is bad, even though the fact may have an unclouded title. But it happens too often that great superstructures have been reared upon a fact which is claimed rather than demonstrated. Facts are like stepping stones; so long as one can get a reasonably close series of them he can make some progress in a given direction, but when he steps beyond them he flounders. As one travels away from a fact its significance in any conclusion becomes more and more attenuated, until presently the vanishing point is reached, like the rays of light from a candle. A fact is really influential only in its own immediate vicinity; but the whole structure of many a system lies in the region beyond the vanishing point.

Such "vain imaginings" are delightfully seductive to many people, whose life and conduct are even shaped by them. I have been amazed at the large development of this phase of emotional insanity, commonly masquerading under the name of "subtle thinking." Perhaps the name is expressive enough, if it means thinking without any material for thought. And is not this one great danger of our educational schemes, when special stress is laid upon

training? There is danger of setting to work a mental machine without giving it suitable material upon which it may operate, and it reacts upon itself, resulting in a sort of mental chaos. An active mind, turned in upon itself, without any valuable objective material, certainly can never reach any very reliable results. It is the trained scientific spirit which recognizes that it is dangerous to stray away very far from the facts, and that the farther one strays away the more dangerous it becomes, and almost inevitably leads to self-deception.

It is such an attitude of mind that scientific training is contributing to the service of mankind. This does not mean that all scientific men exhibit this attitude to the full, but that it is their ideal. This ideal has realized some tremendous results during the last half century, and there is every evidence that it is accumulating momentum for a much larger expression. Compared with this contribution, the material usefulness of science seems tawdry. In general, the world's standards of usefulness are tawdry, but education ought to correct them rather than maintain them.

The conclusion is that all science is immeasurably useful, from fundamental to superficial, on the material plane and on the intellectual plane; and that in these two regions of human need it is the most valuable practical asset the world possesses.

JOHN M. COULTER

BOTANY IN ITS RELATIONS TO AGRICULTURAL ADVANCEMENT[1]

FEW things are more interesting to one of a philosophic cast of mind, especially if he be something of a botanist or agriculturist, than a growing collection of plant varieties. However sluggish of intellect one may be, such a collection—representing forms developed in the long history of the cultivator's art—is sure to excite one's interest regarding their origin. At first thought it would seem that as practically all of the numerous varieties that exist in cultivated plants have been developed as it were under the eye of the grower, we should have a pretty clear understanding and agreement as to their mode of origin. Yet few subjects have proved more perplexing. The stock answer of the breeder or gardener to one's inquiries is usually embodied in the words *sports* and *hybrids*. Is this answer adequate? The enormous importance of the subject, it would seem, should have incited the most intensive study into the problem. Few plants in their ordinary wild forms will repay cultivation. It is only through their improvement that a permanent agriculture became possible. The very baffling nature of the problems presented, instead of attracting students, seems to have repelled them. Systematic botanists have looked upon cultivated plant varieties as artificial products—useful, no doubt, but utterly subversive to notions of classification obtained from plants in their natural habitats. Therefore, they have been neglected and no plants are so rare in museum collections as our common cultivated ones. Such a thing as a reasonably complete herbarium of cultivated plant varieties nowhere exists. The natural result of this has been that the systematic botany of cultivated plants is in woeful confusion. As a rule, numerous botanical species have been based on purely agricultural varieties, but in some cases the opposite extreme is found and perfectly distinct species are confused as garden varieties. As a natural consequence of this neglect by botanists, the great mass of information we have concerning any cultivated plant is largely

[1] Address of the retiring president before the Botanical Society of Washington, March 5, 1910.

the work of men of little or no botanical training.

With the establishment of the numerous agricultural experiment stations in all parts of the world, the doors were opened wide to scientific men to work for the advancement of agriculture. It is instructive to review the general trend of what took place in the fields of agronomy and horticulture, which, broadly speaking, not only cover the whole subject of crop plants, but soils as well. Generally speaking, there are four potent and more or less controllable factors which affect the yield of crops. These are *tillage, fertilizers, rotations* and *variety of plant.* To these might be added the prevention of loss by diseases or insects. Broadly speaking, three types of scientific men went into agronomic work. First, those who were interested in the study of fertility. For the most part, these men were and are chemists and they have studied their problem largely or wholly from a chemical standpoint. Probably as a result of their chemical training the field plot work of these investigators is by far the most accurate agronomic field work conducted. The theoretical side of the subject of soil fertility has recently been stimulated by vigorous attacks on the long-accepted theory of available plant food—an explanation so luminously simple that a few pages of text sufficed to tell the whole story. It may devoutly be hoped that a renewed activity in the study of fertility may stimulate botanical work on the nutrition side of the problem—which is pretty nearly where Sachs left it forty years ago. The second class of scientific men who were attracted to agronomic work were botanists. In large measure, these men undertook investigations dealing with plant diseases, with the end in view of preventing or curtailing the serious losses resulting from such causes. The results of their work furnish the best contributions that botany has thus far conferred on agriculture in this country. So far as field crops are concerned, there are decided limitations to the use of any direct preventive methods such as spraying. As a natural result, investigators of the diseases of such plants were forced to adopt one of two lines of approach to the solution of the problems involved. They could either seek for immune or resistant varieties or they could make a comprehensive study of the crop and the disease and endeavor by such indirect methods as rotations to curtail the disease loss. In either case the result was that the pristine pathologist often graduated into an agronomist. The third class of men who went into crop investigations were generally termed agriculturists and horticulturists. They constituted by far the most diverse group. In a few cases they were simply good farmers. In some cases they were men of very broad training. For the most part they were men with good general equipment. To these men fell the great bulk of the field work involving principally investigations into tillage, rotations and the testing of crop varieties. It thus fell largely to this third class to investigate the complex problems of plant varieties. Even in the few cases where experiment-station agriculturists and horticulturists had good botanical training, the diverse problems facing them as well as paucity of literature gave little opportunity for far-reaching studies. Generally speaking, one of two plans was pursued. In the one case a series of varieties was grown, and all but a few of the apparently most promising were discarded without further ado. In the other case more or less full information was

published regarding each of the varieties tested. Further investigations have clearly revealed the very superficial nature of most of these varietal studies. In general, the collections consisted of such varieties as could be gathered locally and through seedsmen. In only a few cases have specimens been preserved, so that it is not possible now to verify or determine the varieties grown, though in many cases it is certain from the notes that the variety published on was not true to name. There has thus been placed on record a mass of misinformation regarding many varieties. In my opinion, at least fifty per cent. of the varieties that have been published upon are either untrue to name or unidentifiable. I hope I may not seem to be pessimistic in portraying the present status of much of the published information on crop varieties. It is the natural result of neglect by men of proper training to do accurate work of a purely botanical character. As an indirect result of this failure by botanists to apply their trained skill to the problems of agriculture, especially as concerns knowledge of crop varieties, there has arisen the idea that training in systematic botany can not be of particular assistance to agriculture. Therefore, it has all but disappeared from college curricula at least in a form to train students to know plants. Few agronomists and horticulturists graduating to-day from our agricultural colleges are well trained in botany—indeed so far as I know no college is training botanists to enter agricultural work, excepting along pathological lines.

I do not feel that I should be justified in thus painting so gloomy a picture of botany in its relation to agriculture, if the recent trend of things did not indicate that better times were coming—indeed are here. There was one field of work that both botanists and agriculturists entered upon in the course of their investigations that has brought them together, namely, plant breeding. It is a happy coincidence that at practically the same time the interest of all biologists has been stimulated to renewed interest in the problems of variation and heredity. The practical results already obtained by plant breeders is an earnest of what may reasonably be further expected. Incidentally but inevitably, the work of the plant breeder has stimulated interest in the matter of existing crop varieties as well as in the principles underlying variation and heredity. Breeding is, after all, largely the production of new varieties. Thus far, breeders have used for the most part locally established varieties as the basis of the work. This is sound as far as it goes, as the local varieties undoubtedly represent the best adapted of those tried, the poorer sorts having been discarded. It is safe to say, however, that but a small per cent. of existent varieties have been tried in most places—so that there may easily exist varieties superior at least in certain characteristics. A realization of this has led to a clearer appreciation of the value of a comprehensive study of the whole botany of our principal crop plants. This does not mean merely a categorical list of existent varieties—which it is evident can be indefinitely increased by hybridizing—but a sufficiently exhaustive study so that we may thoroughly understand the characteristics, both good and poor, that are available to the breeder. The task is by no means an easy one. In the first place, the number of varieties in all our crop plants is far greater than has commonly been realized. For example, there are probably about 2,000 varieties of wheat, 1,000 of beans, 5,000 of apples, 200 of sor-

ghums, etc. What is needed is not so much descriptions and detailed classification of these varieties, as a classification and understanding of their principal hereditary characteristics. In other words, the knowledge of them needs to be arranged not only with regard to the existing forms, but also as far as possible with regard to their characters and potentialities. Such a monograph does not exist for a single one of our principal crops, though there is an increasing number of contributions to the subject. The field is a vast one in which there is not only a great work to be done in compiling what is known of our cultivated plants, but a greater one in clearing up the many problems concerning their origin.

In a very different way plant breeding is beginning to do much to better agronomic methods. I have before stated that the most accurate plot work being done in this country is by the plots devoted to fertility investigations. How accurate are these? Hall, of Rothamstead, thinks no results with fertilizers are at all trustworthy unless the yield difference is at least 10 per cent. In much of the American breeding work going on 10 per cent. increase by selection would be deemed good progress. The question is, can any feasible system of trial plots measure accurately such a difference? Very recently several men have looked into this subject, more or less independently. The most comprehensive work has been done by Lehmann at the Mysore Experiment Station, India. Similar work has been done by Lyon at Cornell, Montgomery at Nebraska, Shoesmith in Ohio and Smith at Illinois. All of these investigators find a surprising difference in plots due to differences in soil. On what was considered the most uniform soil at the Nebraska Experiment Station the variation between plots on one acre was 35 per cent.—a much greater difference than the breeder of wheat expects to get. Lehmann found differences varying from 0 to 300 per cent. —and further that on many plots the difference was increased or diminished according to the season. He proposes to use in his work with fertilizers only the plots that give uniform results for at least two similar seasons, a method that he calls *standardization*. In this country agronomists have used mainly the system of check plots—a system which it now appears may be absolutely misleading. Indeed, a study of the check plot records in various experiments shows that they vary in just the way that Lehmann found his plots to vary.

Some American agronomists are employing the method of duplicate plots—a plan that is rapidly growing in favor. The number of duplications for the most accurate work will necessarily vary according to the evenness of the soil, four to six duplications apparently being necessary for very accurate results even on fairly uniform soil. The subject is, however, one that needs much additional investigation, as the disturbing effects of soil inequalities have evidently been greatly underestimated.

The results of plant breeding seem likely, therefore, to have a profound effect on agronomy as a whole, demanding as it does both the most accurate plot methods to determine relative yields and a much more intensive knowledge of our crop plants—the material with which breeding must work.

There is still another botanical method that needs to be brought more intensively into agronomy—namely, the method of pure cultures, which has brought so great results in our knowledge of the lower plants. It is this method that enabled

Mendel to discover the phenomena that bear his name. Practical plant breeders now generally use the plant-to-row or centgener method in comparing the value of selected plants. It is probably due to the non-use of such careful methods that the origin of most cultivated varieties is so obscure. In many cases, a so-called sport or hybrid turns out to be a well-known thing—in all probability the result of a stray seed. This is perhaps unavoidable, as the business of the seed grower does not readily lend itself to accurate scientific methods.

Of late years our knowledge concerning hybrids and the behavior of characters in hybrids has increased greatly due to the rediscovery of Mendel's laws and the immense amount of splendid investigation which was thus stimulated. No more admirable body of work has ever been done than that of the Mendelists. If it continues as rapidly as it has we may soon expect to know approximately the extent to which hybridizing is a factor in the evolution of our cultivated plants. While the methods of the practical breeder are perhaps necessarily different or at least less accurate than those of the scientific breeder, yet the results of the scientific work are already having profound effect on practical methods.

Without at all minimizing the fruitful results and greater promises of Mendelian investigations, the subject of sports is to both the breeder and the evolutionist a matter of far greater moment. Certainly our knowledge concerning sports is far less than that of hybrids. The more enthusiastic Mendelists have evinced some disposition to deny the existence of "sports" in the commonly accepted sense and would explain them as the result of some previous, even remote, cross. But it is self-evident that hybrids presuppose the existence of two different things to cross, and sporting is supposed to be one method by which a distinct form more or less suddenly arises. Let us examine carefully the evidence regarding "sports." Bud sports, where one branch of a plant is different from the rest, occurring commonly as variations with differently colored flowers, different leaves, etc., are well known. There can be no question as to the origin of the sport here, though to be sure the parent plant may be a cross or hybrid. Seed sports are supposed to arise in an analogous manner. The general occurrence of certain types of assumed sports is strong argument in favor of their actuality. Thus, white-flowered variants are known in practically all plants with normally red or blue flowers; cut-leaved varieties are very common and generally distributed among the plant families; dwarf varieties occur in numerous species, as do smooth varieties in hairy species and vice versa. The logical inference is that the difference is due in each case to the same underlying cause. In some cases the origin of these sports is a matter of definite record, as in the case of the cut-leaved form of *Chelidonium majus*, the globose-podded form of shepherd's purse and others. In the white-flowered form of bleeding heart—its only variant—previous hybridization seems clearly excluded by the absence of any related form that will cross with it. Many such cases can be enumerated and tend to uphold clearly the gardener's idea of sports. But what are these sports, and how do they arise? Apart from the fundamental idea that they are large and permanent variations, breeders and gardeners in general attach three other ideas, namely, that high nutrition and other extreme conditions favor sporting; that many plants must be

cultivated a long time before sporting is induced, and that in any case sports are actually or relatively very rare. Will these ideas stand the test of scientific scrutiny experiments? It is evident that these problems are of high importance both to evolutionists and to agriculturists. De Vries with his *Œnotheras* and his theory of mutation as the chief factor in evolution has particularly interested the scientific world in these phenomena. He has worked out in great detail the facts of variation as they occur in the evening primrose and makes a strong case for his theory. Recent cytological study of the *Œnothera* mutants or variants shows that one of them has twice as many chromosomes as the others; in other words, that this mutant at least has suffered a pronounced change in its hereditary mechanism. It is only natural that this should at once have aroused the suggestion that perhaps all sports or mutants are the result of more or less marked derangement of the hereditary mechanism, by which a character or factor of some sort is gained or lost. MacDougal's work in subjecting very young ovules to chemical influences, and Gager's similar experiments with radium emanations, are also reported to have yielded marked variations, perhaps sports. Tower also secured true sports in increased numbers from his Colorado potato beetles by subjecting them to untoward conditions of heat and moisture during breeding. In this case, however, all the sports secured were previously found occurring naturally. There is a tempting subject here for speculation—indeed one that has been assiduously tilled, but to follow it up will lead us too far afield. The limited historical and experimental evidence of a critical character clearly upholds, however, the reality of sports.

It is an illuminating fact that most of the information concerning the origin of cultivated plants and animals is that brought together long ago by Darwin. Recently De Vries has gathered much additional data. Both these men sought the facts primarily in support of a theory. Scientific men are usually more concerned in finding an explanation of phenomena than in gathering the facts. But we can not all be philosophers and theorists—indeed, the principal difficulty with biological science is that we have a plethora of theory and a dearth of critical facts. Especially is this true in the subject of biological evolution, where nearly every possible guess and combination of guesses as to the actual method of evolution has been made. Where such guesses or theories stimulate additional inquiry they are valuable—otherwise, they are useful only to practise mental gymnastics. It is the great merit of many recent investigators, De Vries in particular, that they emphasize the importance of experimentation. De Vries's work bristles with suggestive lines of experimentation mostly bearing on the subject of the origin of cultivated plants, and nearly all of practical importance in agriculture as well of great interest in themselves. If any one believes that there is any immediate likelihood of biologists agreeing on evolution, all he has to do, using the slang of the day, is to start something. However much agreement there may be on the facts—there is sure to be violent disagreement on the interpretation of the facts. For example, De Vries and others believe that sports which usually breed true from the start are intrinsically different from ordinary or fluctuating variations induced by soil or otherwise and which have no effect on the offspring. On the other hand, Tower, who has conducted extensive investiga-

tions in the evolution of the Colorado potato beetle and its relations—work comparable to that of De Vries on *Œnothera*—argues strongly to show that his sports or mutations differ from fluctuations only in degree, not in kind. By definition, if the variant transmits its characters fully it is a mutation or sport; if not at all, it is a fluctuation. But many supposedly fluctuating variants transmit their characters in large part at least temporarily. Thus peas grown on warm or sandy soils are said to become mature earlier than the same variety planted on colder soils—and this difference is transmitted at least to their immediate progeny. It is believed to be in virtue of this supposed type of variation that northern grown seeds like corn often possess increased earliness when planted south; that continued selection as in sugar beets is necessitated to keep the plants to a high standard. Such plants clearly transmit to their progeny characters limited in both amount and duration. Are they then fluctuations or mutations? Those who hold that fluctuations have no effect at all on heredity, suggest that the sugar beet and kindred cases may represent complex polyhybrids continually breaking up and that rigid selection would, therefore, result in securing pure constant lines with high sugar content. Many mutations are at first partial, as in the cases of many double flowers. The first suggestion of doubling is often only a single additional petal. In the progeny of this individual some with more petals nearly always occur—and the process eventually results in full doubling. The general progress in these cases is seemingly parallel to what occurs in securing the pure lines out of a complex hybrid. A similar case if true is found in Burbank's red *Eschscholtzia*—the first hint of which was a red streak in the petals of a yellow sort. By continued selection the pure red was isolated. Professor Setchell tells me, however, that red-flowered eschscholtzias occur wild in certain localities in California. There is room for much discussion on all these points—but their settlement requires a larger body of critical facts than are yet available. There are plenty of gardeners' accounts of such phenomena to be had and they are probably true, but they do not possess scientific accuracy. Along these lines there is presented an alluring field of botanical work.

A clearer understanding of the different types or degrees of variation is most important. De Vries would recognize only three types, namely, fluctuations, mutations and ever-sporting plants. The latter include mostly plants with variegated leaves or flowers—which also constantly bear part of their leaves or flowers without variegation. A common example is the variegated-flowered larkspur. The azaleas with flowers on some branches red, on others white or striped, offer perhaps a similar phenomenon.

It is quite certain that such a classification simplifies the matter too much. Johannsen's work with beans clearly shows that mutations are often very small, even minute—but they are inherited—while similar variations not inherited are considered fluctuations.

De Vries's compilation of available evidence on the origin of plant sports tends to uphold in general the idea of the gardeners—namely, that sports are comparatively rare; that unusual conditions, especially of nutrition, favor their occurrence; and that often a plant must be cultivated a long time before it will sport. His evidence further shows that in some cases breeders sought out natural sports—and merely intensified their characteristics by cultivation. Whether De Vries's

theories are correct or not, wholly or partly, is of far less importance to agriculture than the stimulus he has given to the experimental study of plant variation. Not only has he done a vast amount of this sort of work himself, but he points out very clearly numerous problems awaiting the investigator.

It is remarkable that thus far so little has been done in attempting to produce anew the varieties of cultivated plants by beginning with the wild plant and conducting the work under critical scientific conditions. This is perhaps impossible in the case of our most important plants which have been cultivated since prehistoric times—and of whose original form we are in many cases ignorant, but it surely is a feasible and logical method of procedure in the case of plants domesticated in recent times, as is the case with many ornamentals. There is, I believe, no dissent from the statement that cultivated plants show far greater diversity than their wild progenitors. Is this greater diversity merely due to intensification of differences already possible of discernment in the wild plant, or do really new types appear under the stimuli of cultivation? To use a simple example, *Impatiens sultani*, an African ornamental, was first introduced into cultivation about twenty years ago, only a single color being then known. It now occurs in four distinct colors. Have these arisen under cultivation or were they found as wild sports? A more complex case. *Phlox drummondii* is a native to Texas and not very variable, so far as known only pink, purple and red varieties existing wild. It was introduced into cultivation about seventy-five years ago. There is now a bewildering array of color varieties—both with entire and with fringed petals. In the so-called star of Quedlinburg varieties the central tooth of the fringed varieties is prolonged into a lobe as long or longer than the petal. In the wild form there is apparently no hint of such a character. It ought to be no difficult task to repeat the evolution of these forms under test conditions and thus get a full record of what takes place. Until this is done our picture of the process must remain incomplete. How far extreme conditions as to soil, heat, moisture and other external factors may affect the process of variation, especially permanent variations, is one of great interest and importance. Our wide range of soils and climates gives us unusual opportunity to plan such investigations. To start anew with the wild forms of our most important crops, wheat, oats, corn, beans, potato, etc., is rendered difficult owing to our ignorance of the wild progenitors of these crops. Why these should have disappeared if such is the case is very puzzling. Aaronsohn has recently discovered in the mountains of Palestine what are probably the wild originals of wheat, of barley and of rye. As this country was long ago well explored botanically, the question at once arises—why were not these plants found? Aaronsohn offers a humorously simple explanation, namely, that no botanist ever collects a cultivated plant and no agronomist ever looks at a wild one. Perhaps a similar explanation may account for our ignorance of corn and other American natives in the wild state. A particular interest in knowing the wild form of such plants is to be able to measure the progress that has been made by cultivation. Another is to determine how quickly it may be possible to breed up to the approximate standards of the long-cultivated strains. There is a general belief that great improvements can be made in the early processes of breeding for improve-

ment but that these rapidly and progressively become less and less with each step in advance. This is perhaps true as it is a general law of nature. Yet the improvement made in some supposed cases is vastly greater than could possibly have been anticipated. Thus the gap from Johnson grass to its supposed derivations, such as Kafir, Jerusalem corn, milo, Sumac sorghum and a host of other varieties is so great as to stagger one's belief. Yet the botanical evidence is good enough to warrant critical experimental investigation.

How much further wheat, corn and other long-cultivated plants may still be improved can not be foretold, because we are too ignorant of the potentialities which have brought them to their present development. In any attempt that may be made to redevelop the cultivated forms from the wild forms, two things will have to be considered—first, that various forms of the wild plant may and probably do exist in different regions—and second that even beginning with the same wild form its descendants in different regions will probably vary in different directions. Only on one or both of these hypotheses can we explain the fact that with anciently cultivated plants each region has its own peculiar varieties and types. The problem of the origin of the more marked varieties of the plants cultivated in and since prehistoric times becomes an exceedingly complex one, probably capable of being duplicated only in small part. We must not underestimate the ability of even very low races of agricultural people to improve their cultivated plants. Certainly the Indians developed corn to a very high degree and had some pretty clear ideas regarding its culture. For example, the Virginia Indians made it a point to plant in each hill seed from several different ears.

It seems to me that we too often err on the side of making phenomena appear more simple than they really are. Plants are vastly more complex organisms than our formulated ideas recognize. Many of their phenomena completely baffle us. For example, I might mention what has been called aggressiveness in a plant—namely, its ability not only to occupy and maintain the soil, but to spread and crowd out other plants. This is particularly evident in plants introduced from one country to another. Thus nearly all of our weeds are of old world origin. The same is true of our permanent meadow and pasture plants, where ability to occupy and hold the ground against weeds is essential. In this respect our American grasses and clovers utterly fail before the foreign immigrants. Some other striking instances of the great aggressiveness of an immigrant may be cited. The introduced English violet is said to be the worst of weeds in Mauritius; American cacti are becoming a pest in South Africa; the marvelous vigor and spread of the American waterweed (*Elodea*) under European conditions is well known. Several explanations of these and similar phenomena have been advanced. The commonest one is that the plant is introduced but its fungous and insect enemies are not. Therefore, the plant is released from all handicaps as it were and can exercise to the utmost its inherent energy. A second and related explanation is that every plant becomes held within limits by the competition of other plants in its native land, and very often in the new environment the native plants do not have an equal restraining influence—because they have had to contend with a different set of competitors. A third idea is that any organism with the ability to

spread at all becomes more energetic through the constant mixing of blood of the advancing population. All these ideas are interesting, but difficult, if not impossible of experimental proof. The last suggestion receives some support from the fact that many weeds and other organisms "peter" out after they have ceased to spread. The recent examples of the Russian thistle and the prickly lettuce are familiar cases. Such phenomena may be due wholly or in part to increase in enemies —but in many cases like the two cited there is no iota of positive evidence. I think we ought to give such phenomena more consideration, as they reveal traits in plants that transcend all of our stereotyped and inadequate theories. The old gardener often treats his plants as if he regarded them as sentient beings. Perhaps we err in considering them too much as machines.

I have touched thus much on the botany of our cultivated plants and their origin and behavior under domestication because I believe that there lies here a great field for botanical and agricultural advancement. It matters not what we call this phase of botany—its successful prosecution demands both broad and intensive botanical training. It requires at least a good knowledge of systematic botany, of plant physiology and of the theories and principles of plant breeding and plant evolution. One must at least know all the botany possible of the plants he is immediately concerned in breeding, lest he be lured into needless error. Among his many experiments, Mr. Oliver has made some very interesting hybrids of *Poa arachnifera*, the Texas bluegrass and Kentucky bluegrass, a circumboreal plant. His culture soil was presumably sterilized, yet mixed with his hybrids were plants of Canada bluegrass, *Poa compressa*. One enthusiastic Mendelist was jubilant over the supposed discovery of the origin of this grass and at once proposed an additional series of experiments. Now *Poa compressa* is a European species—and the securing it by crossing a Texas species with common bluegrass was certainly a startling phenomenon. Fortunately or perhaps unfortunately, some of the other supposed hybrids in the lot turned out to be other grasses, including timothy and sweet vernal grass, so that the source of the error was evident. It points, however, clearly to the necessity of the scientific breeder knowing the systematic botany at least of the group he is working with.

I well recall that when I first began to study plants I promptly found about a dozen species of red clover—at least they were different from each other. It took a long time to teach me that in plants there were differences and differences, some of which should be taken seriously and others ignored. In general, I was taught that any differences that existed in closely related cultivated plants were to be ignored, but in wild plants they would usually have to be considered. It is really very fortunate for the cultivated plants that systematic botanists have not taken their differences seriously, otherwise we would have chaos indeed. It is unfortunate that the conservatism which most systematic botanists exhibit toward cultivated plants should not be exhibited as well toward wild plants. If more attention had been given to the cultivated plants, think what a vast host of reputed wild species would have escaped the pangs of christening. There used to be hope that after a while all the species would be described—so that systematic botanists could devote themselves to deeper studies. But alas, it seems only necessary to make finer distinctions to reveal a wondrous display of so-called species where

none was seen before. It, therefore, seems inevitable that a new race of systematic botanists will have to be developed to devote themselves to cultivated plants—for it needs no seer to predict that many generations of botanists will be needed to define and describe all the minute forms in nature which it is now proposed to call species. The fatuity of such work, however, will defeat itself. As a matter of fact, the naming of a species is an interpretation of facts just as our theories of variation are interpretations of the same or very similar facts. For both purposes we need far more of the facts that can only be gathered in rigid pedigreed breeding experiments. Botanists have too long neglected the most vital features of botany to the theoretical evolutionist and to the commercial breeders. We have developed to a high degree nearly every phase of the subject that does not touch industry—and have neglected those of most practical import. Our hope of aiding the art of agriculture is in developing its underlying sciences. Too many of us have reversed this idea and think to help the sciences of agriculture by devoting more attention to its art. But gardeners do things with plants that are the despair of the physiologist, and there always will be vastly better farmers than the scientists.

The matter of botanical instruction in all schools is to a large extent a matter of fashion—and the fashion is usually set by the larger universities, where no attempt is made to give botany an industrial trend. There has thus been developed a splendid lot of texts on morphology, embryology, systematic botany, physiology, etc., but none of this material has been presented in its agricultural bearing, and consequently the field of botany in agriculture has not been clear. At the present time it has neither direction nor aggressiveness. What we really need to work on is the science of the breeder's art and the science of the gardener's art. At present, the art is far in advance of the science. In fields where the agricultural art was not highly developed—notably pathology and bacteriology—the botanist has accomplished great things. Greater things remain in the botanical fields he has thus far so largely neglected. If we pursue agriculture or any phase of it without devoting our science to it, we can at most become expert farmers. By devoting our science to agriculture and having faith in its potency, no man can foretell the outcome.

I have endeavored to indicate what I regard as the most promising lines for botanical work to advance agricultural progress. The routes that the investigators have followed and are following along these lines furnish the natural and best possible chart upon which to map botanical courses in agricultural schools. These courses should be fashioned as far as possible to promote interest in the botanical problems of agriculture, rather than those with little or no agricultural contact. To me it seems as if the great field that is at present open to us is that of determining as fully as possible the potentialities of our principal crop plants so that they may be utilized to the utmost.

In some ways we might compare our present knowledge of plant species or their subdivisions to the knowledge of organic chemistry fifty years ago. At that time it was believed that organic compounds could be formed only by vital processes. In a similar way there exists among biologists the more or less unformulated idea that species and subspecies are the result of forces beyond our command; that we can study their evolution but can not control the processes. It seems to me that the results obtained by the cultivator of plants

and the domesticator of animals virtually contradict this idea, enough so at least that there is good basis for De Vries's bold prediction:

> A knowledge of the laws of variation must sooner or later lead to the possibility of inducing mutations at will, and so of originating perfectly new characters in plants and animals. And just as the process of selection has enabled us to produce new races, greater in value and in beauty, so a control of the mutative process will place in our hands the power of originating permanently improved species of animals and plants.

C. V. Piper

Washington, D. C.,
March 5, 1910

SCIENTIFIC NOTES AND NEWS

Following the advice of its advisory board, The Wistar Institute of Anatomy is about to extend its work by the establishment of a department of embryology. At a meeting of the board of managers of the institute, held May 27, a professorship of embryology was established, and Professor G. Carl Huber, of the University of Michigan, was called to this chair. Professor Huber will begin his work at the Wistar Institute in 1911.

Dr. William Colby Rucker, of the United States Public Health and Marine-Hospital Service, has been granted leave of absence for one year to accept the position of health commissioner of Milwaukee.

Dr. C. F. Lorenz, formerly of the Queen's University, Kingston, Ontario, has entered upon the duties of his position as associate physicist in the Physical Laboratory of the National Electric Lamp Association. Mr. A. G. Worthing, of the University of Michigan, and Mr. M. Luckiesh, of the University of Iowa, have also accepted appointments in the laboratory.

Mr. Jerome D. Greene, secretary of the Harvard College Corporation, has been appointed superintendent of the Rockefeller Institute for Medical Research and its new hospital.

Dr. Alës Hrdlička has been promoted to a curatorship of anthropology in the U. S. National Museum. He has started for South America to carry on some work in Peru and Bolivia and to attend the Congress of Americanists.

Columbia University has conferred its doctorate of science on Sir William Henry White, for many years director of naval construction of the British navy, and on Dr. W. J. Mayo, the eminent surgeon of Rochester, Minn.

On the occasion of the installation of the Duke of Devonshire as chancellor of the University of Leeds, the degree of doctor of science was conferred on Lord Rayleigh, Sir Clements Markham and Professor William Osler.

Lord Rayleigh has been promoted from a corresponding to a foreign member of the Berlin Academy of Sciences.

Dr. W. Solomon, professor of geology at Heidelberg, has been elected a foreign member of the Academy of Sciences in Milan.

The two eminent pharmacognosists, Professor Arthur Meyer, of Marburg, and Professor A. Tschirch, of Bern, were elected honorary members of the American Pharmaceutical Association at the recent meeting in Richmond, May 3–7, 1910.

Dr. Rollin D. Salisbury, professor of geographic geology at the University of Chicago, has been elected president, and Dr. Henry C. Cowles, assistant professor of plant ecology, first vice-president, of the Geographic Society of Chicago.

The American Philosophical Society has appointed its president, Dr. William W. Keen, to represent it at the Centennial Jubilee of the University of Berlin to be held in October next.

Professors Sollas and Bowman have been appointed university representatives from Oxford University to the eleventh International Geological Congress, to be held at Stockholm.

The Barnard Medal was awarded at the commencement exercises of Columbia University to Professor Ernest Rutherford, director of the physical laboratories, University of Manchester. This medal, established

by the provisions of the will of President Barnard, is awarded quinquennially for discovery in physical or astronomical science, or the novel application of science to purposes beneficial to the human race, which in the judgment of the National Academy of Sciences shall be esteemed most worthy of such honor. The award of 1895 was to Lord Rayleigh and Professor William Ramsay; that of 1900, to Professor Wilhelm Conrad von Röntgen, and that of 1905, to Professor Henri Becquerel.

THE friends and former pupils of Mr. A. E. Shipley, of Christ's College, Cambridge, propose to present him with his portrait, in recognition of his "services to zoological teaching and research, and his eminent usefulness to the University and to his college." The portrait will be painted by Mr. William Nicholson.

PROFESSOR HELLER, director of the Pathologic Institute of Kiel, celebrated his seventieth birthday on May 1.

MR. FERDINAND ELLERMAN, in charge of the expedition organized by the Astronomical and Astrophysical Society of America to observe Halley's comet in the Hawaiian Islands, reports that in spite of unfavorable weather conditions he has obtained a set of excellent photographs of the comet. No trace of the comet's head could be seen while in transit over the sun, although it was carefully sought under favorable atmospheric conditions.

PROFESSOR R. W. WOOD, of the Johns Hopkins University, Baltimore, who will spend next year abroad, will give, in London, the Thomas Young oration of the Optical Society and the Traill Taylor lecture before the Royal Photographic Society.

M. JEAN CHARCOT has returned to France on the *Pourquoi Pas* from his Antarctic expedition.

DR. CHARLES PEABODY, of Harvard University, has returned from North Carolina, where, during the month of May, he explored two groups of small mounds in Cumberland County, near Fayetteville.

DR. E. GRAWITZ, professor of pathology at Berlin, will visit this country in the autumn and deliver an address before the New York Academy of Medicine on "Pernicious Anemia."

ON May 31, Mr. C. J. Holmes gave the first of two lectures at the Royal Institution on "Heredity in Tudor and Stuart Portraits"; on June 2, Major Ronald Ross gave the first of two lectures on "Malaria," and on June 4, Professor J. A. Fleming began a course of two lectures on "Electric Heating and Pyrometry," these being the Tyndall lectures.

DR. W. S. BRUCE, leader of the Scottish Antarctic expeditions of 1902–04 and 1911, gave a lecture, with lantern illustrations, on "Antarctic Exploration" at Oxford University on May 27.

PROFESSOR VICTOR BJERKNES, of the University of Christiania, gave a lecture on "The Synoptical Representation of Atmospheric Motions," at University College, London, on May 27.

GENERAL CYRUS BALLOU COMSTOCK, U. S. A. (retired), the eminent engineer, member of the National Academy of Sciences, died on May 29, at the age of seventy-nine years.

LIEUTENANT BOYD ALEXANDER, known for his important scientific explorations in Africa, was killed by natives in the Soudan, on April 2.

MR. J. B. N. HENNESSEY, F.R.S., known for his surveys and other scientific work in India, died on May 23, at the age of eighty years.

M. BERNARD BRUNHES, the director of the observatory of the Puy de Dôme, known for his researches in meteorology, has died at the age of forty-seven years.

DR. SALVATORI LO BIANCO, of the Zoological Station at Naples, has died at the age of fifty years.

M. GREHANT, professor of histology of the Museum of Natural History and director of the laboratory of the Ecole de Hautes Etudes, has died at the age of seventy-two years.

WE learn from the London *Times* that a number of visitors inspected the National Physical Laboratory at Teddington on March 18, by the invitation of Sir Archibald Geikie,

the president of the Royal Society, who is chairman of the general board. Those present included Lord Crewe, Lord Rayleigh, Sir J. Wolfe Barry, Sir Joseph Brunner, Sir William White, Sir Philip Watts, Sir Joseph Larmor, Sir John Thornycroft and Sir Gerard Muntz. The report states that last year the income amounted to £24,270, as against £21,871 in the previous year. Of this nearly £2,000 was due to the treasury grant for aeronautical work from June to December, and the fees for tests, etc., carried out rose from £13,088 to £14,240. The executive committee expresses the opinion that the time has now come when the interests of meteorology, terrestrial magnetism, etc., will be best served by separating them from the research and test work of the laboratory, the application of science to engineering, electrotechnics, naval architecture, etc.; and a report to this effect, embodying a scheme by which the change may be effected, has been transmitted to the treasury, by which it has been favorably received. The committee has also prepared a scheme, involving an estimated expenditure of £30,000, for providing the additional buildings required for carrying out this change satisfactorily, and also for increasing the inadequate accommodation for certain departments, particularly metallurgy and the general administration of the laboratory.

We learn from *Nature* that a grant of £100 from the Worts Fund, of Cambridge University, will be made to Mr. E. A. Wilson, of Gonville and Caius College, who has been entrusted with the organization of the scientific department of the British Antarctic Expedition, 1910, towards defraying the expense of the equipment. The scientific staff of the expedition includes Messrs. D. G. Lillie, of St. John's College; E. W. Nelson, of Christ's College; T. G. Taylor, of Emmanuel College; E. A. Wilson, of Gonville and Caius College, and C. S. Wright, of Gonville and Caius College. Grants of £50 to Mr. C. E. Moss, curator of the University Herbarium, towards defraying the expense of botanical investigations which he proposes to make on the continent of Europe, and of £25 to Mr. R. H. Rastall, towards defraying the expense of a visit which he proposes to make to South Africa for the purpose of carrying on geological investigations, will also be made.

The April number of the *Journal of Home Economics* is largely devoted to a discussion of various phases of the school lunch question by nearly a score of writers. There is an article on school feeding in Europe by Miss Louise Stevens Bryant, who is in charge of the School Feeding Inquiry of the Russell Sage Foundation; Dr. Ira S. Wile writes on the general problem, while other articles furnish accounts of experiments that have been made in Philadelphia, New York and Boston. An interesting symposium is published on economy of materials in school lunches, containing in detail the practical working out of the problem in different parts of the country. Ignorance in the homes of the poor as a contributing cause of malnutrition of the children is a subject treated by Miss Gibbs of New York and Miss White of Baltimore, together with the remedy which has already proved effective in New York, that is, the work of the visiting dietitian. The American Home Economics Association which publishes the *Journal of Home Economics* aims "to improve the conditions of living in the home, the institutional household and the community," and unites all actively interested in home problems.

UNIVERSITY AND EDUCATIONAL NEWS

At the commencement exercises of Bryn Mawr College it was announced that the college had obtained money sufficient to pay its debts, and in addition $250,000, which entitled it to the appropriation of $250,000 of the General Education Board. The sum raised by the Alumnæ Association was $304,900 which is to be used for the endowment of chairs in mathematics, English and economics.

The legislature of Maryland has made an appropriation of $25,000 a year for 1911 and 1912 for the Johns Hopkins University.

The Jefferson Medical College has purchased the building formerly occupied by the Pennsylvania College of Dental Surgery at

Eleventh and Clinton streets, Philadelphia, and will use it for laboratory purposes.

THE late Sir Donald Currie's daughters, Mrs. Mirrilees and Mrs. Percy Molteno, have given a sum of £25,000 to the University of Cape Town for the construction of a hall as a permanent memorial to Sir Donald Currie.

THE alumni of Brown University by a vote of 2,008 to 223 favor the removal of the denominational restriction which requires the president and the majority of the trustees to be baptists.

IT is reported that from the answers to several hundred letters sent by Yale University to heads of preparatory schools and public high schools, the majority favor science and history as substitutes for Greek at the entrance examinations of the academic department. The changes will, it is said, probably be adopted at the entrance examinations in 1911.

DR. GEORGE BLUMER, professor of medicine, will succeed Dr. Herbert E. Smith as dean of the Yale Medical School.

MR. H. N. EATON, instructor in geology in the University of North Carolina, has been appointed to a similar position in the School of Mines, University of Pittsburgh.

SCIENTIFIC BOOKS

Charles Darwin and the Origin of Species. By E. B. POULTON. London and New York, Longmans, Green & Co. 1909.

Professor E. B. Poulton, Hope professor of zoology in Oxford University, has long been known as the leading proposer and defender of theories of mimicry, warning, directive and recognition coloration and the like. Next to the names of Bates and Müller, which are names of the pioneer observers and hypothesis makers in this field, stands the name of Poulton.

The name must now be associated with another distinction; it is that of the most loyal present-day disciple of Darwin. Poulton is a whole-hearted accepter and ardent defender of everything that came from the mouth and pen of his immortal master. There are no mental reservations about Professor Poulton's Darwinism; no interpretations other than the obvious ones; no buts nor howevers.

In his addresses (which I have referred to recently in other pages of this journal) at Baltimore in January, 1909, before the American Association for the Advancement of Science, and at Cambridge in June of the same year at the great Darwin Commemoration Meeting, Professor Poulton was the conspicuous exception among the other speaking biologists in his unreserved acceptance and defence of Darwinian selection as the all-important factor in species-forming and evolution. He now appears as the author of a book explicitly given to the exposition and defence of this factor and to the answering of its critics.

The book comprises the two addresses already mentioned, together with two lesser ones given as banquet speeches on the same general occasions; another given at the Oxford Darwin celebration in February, 1909; an anniversary address given in December, 1908, before the Entomological Society of America in Baltimore; a group of about twenty hitherto unpublished letters written by Darwin to Roland Trimen between 1863 and 1871; and four brief appendices including notes on Darwin and the hypothesis of multiple origins, Darwin and evolution by mutation, Darwin's health and work, and De Vries's fluctuations as inconsistently treated by certain English believers in them. The whole collection is the consistent utterance of a perfect Darwinian.

The new Darwin letters do not add much to our knowledge of the master's personality, but they are interesting. They are full of glimpses of hard and constant work and continuous and interfering ill health. They touch especially—and this is their particular interest to Professor Poulton—the subject of color and pattern (Trimen was a devoted observer in this field). All the references to this subject are, however, tinged with the sexual selection hypothesis which was more importantly in Darwin's mind than any hypoth-

esis of mimicry or the like. There are some very quotable bits in the letters. In a letter of April, 1868, Darwin writes:

> Many thanks for your Photograph, and I send mine, but it is a hideous affair—merely a modified, hardly an improved, Gorilla.

Mr. Trimen's first meeting, or rather first seeing, of Darwin, as described by him in a letter to Professor Poulton, is an interesting reminder of the reality of the heresy of the "Origin" in its first days.

> It was in the Insect Room of the Zoological Department of the British Museum that I had my first glimpse of the illustrious Darwin. Towards the close of 1859, after my return from the Cape, I spent much time in the Insect Room identifying and comparing the insects collected with those in the National Collection. One day I was at work in the next compartment to that in which Adam White sat, and heard some one come in and a cheery, mellow voice say, "Good morning, Mr. White; I'm afraid you won't speak to me any more!" While I was conjecturing who the visitor could be, I was electrified by hearing White reply, in the most solemn and earnest way, "Ah, Sir! if ye had only stopped with the 'Voyage of the Beagle'!" There was a real lament in his voice, pathetic to any one who knew how to this kindly Scot, in his rigid orthodoxy and limited scientific view, the epoch-making "Origin," then just published, was more than a stumbling block—it was a grievous and painful lapse into error of the most pernicious kind. Mr. Darwin came almost directly into the compartment where I was working, and White was most warmly thanked by him for pointing out the insects he wished to see. Though I was longing for White to introduce me, I knew perfectly well that he would not do so; and after Mr. Darwin's departure White gave me many warnings against being lured into acceptance of the dangerous doctrines so seductively set forth by this most eminent but mistaken naturalist.
>
> A little while afterwards, on the same day, I again saw Darwin in the Bird Galleries, where it was, I think, G. R. Gray who was showing him some mounted birds. A clerical friend with me, also a naturalist, curiously enough echoed White's warning by indicating Darwin as "the most dangerous man in England."

The most interesting of Professor Poulton's personal contributions to his volume are two papers treating the special subject of his studies, namely, the addresses on "The Value of Color in the Struggle for Life" and "Mimicry in the Butterflies of North America." One is a suggestive general treatment of the use-of-color subject, the other a detailed special consideration of a suggestive set of illustrations of one phase of this subject. As an entomologist acquainted somewhat with the alleged mimicry case from the Pacific Coast which to Professor Poulton seems to be, if really proved, "one of the most interesting and instructive examples of mimicry in the world," viz., the resemblances between *Limenitis californica* and *L. lorquini*, I can only say that much more evidence than at present has been collated is necessary before this case can receive general acceptance. But this Professor Poulton also recognizes fairly, so any present hesitancy to see the pertinence of this example of mimicry can not be misconstrued by its sponsor. What is needed in this case is exactly stated by Professor Poulton, viz., "extensive investigations in America."

V. L. K.

Stanford University, Cal.

Illustrations of African Blood-Sucking Flies other than Mosquitoes and Tsetse Flies. By Ernest Edward Austen, Assistant in the Department of Zoology, British Museum (Natural History), with colored figures by Grace Edwards. London. 1909. Pp. 221, 13 colored plates.

Repeated demonstration of the agency of blood-sucking insects in the transmission of certain diseases invests with the greatest practical importance an accurate knowledge of the genera and species of these forms. Warfare against such diseases is now being carried on with great vigor in Africa and the volume under consideration has been prepared with a view to aiding in this contest.

In the preface the author mentions the plan of a general monograph on the blood-sucking insects which was originated by Sir E. Ray Lankester, when director of the natural history departments of the British Museum. Four volumes on mosquitoes, by F. V. Theobald,

were issued between 1901 and 1907, one, in which tsetse flies were treated by E. E. Austen, in 1903, and now this volume by the same author covers the remainder of the Diptera.

With the exception of Egypt the territory covered in this work falls within the limits of the zoogeographic province ordinarily called the Ethiopian region. The record is confessedly incomplete, even for the region indicated, as the material available was at best scanty, so that data concerning detailed distribution which are given in the last chapter of the book are of relatively little value. This defect, which is commented upon briefly only in the preface, is of a serious character, since many of the medical and military men who will be called upon to use the book are likely to draw unwarranted, though none the less unfortunate, inferences from the brevity of the records, but even more serious difficulties arise from the omission of any reference to those species not illustrated here.

As natural in a work dealing with forms that have so recently attracted particular attention, museum material from different countries is sure to be variable in amount and the records compiled therefrom of very unequal value. Cape Colony naturally leads in number of species recorded and Uganda is a close second, but some states have only three or four species listed, *i. e.*, are represented by very little material in the museum collections and yet the text of this chapter conveys no hints as to the proper method of interpreting its lists.

Of the Chironomidæ the work describes and figures one genus including three species; of the Simuliidæ, one genus with four species; of the Psychodidæ, one genus with a single species; of the Tabanidæ, seven genera with eighty-four species; of the Muscidæ two genera with five species, and of the Hippoboscidæ one genus with three species. These represent less than one half of the African species already known. The illustrations are very successful and in practical work will be of immense value. Synoptic keys as well as specific and generic descriptions are entirely omitted and reliance placed rightly upon the accuracy of the figures which are admirably done. The habitus and coloration of the species figured are vividly represented, even though few structural features are distinguishable in the plates.

The author handles the bionomics of the group treated in the broadest possible manner, always from the point of view of disease dissemination, and the records of work done by other investigators are particularly full and well digested. In fact, the text is a mine of information concerning the breeding, feeding habits, appearance and relation to disease of the individual families, genera and species. The work is evidently well done and bears the earmarks of accuracy. It also stands the test as regards completeness of data concerning the species treated.

The book is certainly popular—in the best sense of the word—rather than scientific, and is sure to prove very valuable to investigators experimenting on suspected species in the field. It is also an important reference work for those interested in this group either as students of Diptera, of medical zoology, or of disease transmission through insects.

HENRY B. WARD

Aposporie et Sexualité chez les Mousses, II. Par EL. et EM. MARCHAL. Bull. de l'Acad. roy. de Belgique (Classe de sciences), No. 12, pp. 1249–1288. 1909.

In previous papers on mosses the Marchals have shown that the differentiation of sex in certain diœcious species takes place in the formation of spores in the sporangia, a single sporangium containing both male and female spores; further, that a regeneration obtained from the cells of the sporophyte of a diœcious species before the formation of spores will develop into an hermaphroditic growth and produce both archegonia and antheridia.

The present paper deals with the sexual character of the products of apospory or these sporophytic regenerations. As might be expected, the aposporic outgrowth induced from a mutilated young sporangium is found to agree with the sporophyte in the number of chromosomes in its cells, and with $2n$ chromo-

somes may therefore be classed as a diploid growth in distinction to the original gametophytic or haploid stage with its $1n$ number of chromosomes. These diploid growths of diœcious (heterothallic) species remain entirely sterile, though producing apparently normal antheridia and archegonia. Attempts made to produce hybridization between these hermaphroditic diploid growths and male and female plants of the normal $1n$ generation resulted in failure.

With hermaphroditic (homothallic) species the condition is different and the aposporic outgrowths are fertile. Their gametes with $2n$ chromosomes unite and produce sporophytes with $4n$ chromosomes. These tetraploid sporophytes form spores with again $2n$ chromosomes, which grow into fertile gametophytes with double the normal chromosome number, thus producing a definitely fixed bivalent race (*e. g., Amblystegium serpens bivalens*). The regeneration from the tetraploid sporophyte gives rise to a race with $4n$ chromosomes which as yet has remained sterile. A sporophyte with $8n$ chromosomes might be produced if this $4n$ race could be induced to fruit.

No phenomena have been observed, such as apogamy or supplementary chromosome reduction, which would avoid the doubling of chromosomes in the races obtained from sporophytic regenerations.

A rather careful series of measurements were made of the size of the nuclei and cells in the different stages obtained, and it was found that the volume of the cells and of the nuclei were directly proportional to the number of chromosomes in the $1n$, $2n$ and $4n$ gametophytes as well as in the $2n$ and $4n$ sporophytes. Further, it was seen that this increase in size of the cells with an increase in the number of chromosomes resulted in the enlargement of certain organs such as the antheridia and archegonia.

The Marchals believe that apospory is likely to occur in nature from wounding of the sporophyte and that bivalent races have thus been formed.

There is promised a continuation of these investigations on the mosses which have proved already of such great interest to the students of sex.

A. F. Blakeslee
Connecticut Agricultural College,
Storrs, Conn.

A List of Geographical Atlases in the Library of Congress, with bibliographical notes. Compiled under the direction of Philip Lee Phillips, F.R.G.S., Chief Division of Maps and Charts. In two volumes, cloth: Vol. I., Atlases, pp. xiv + 1,208, Vol. II., Author List, Index, pp. 1,209–1,659. Washington, Government Printing Office. 1909. $2.35.

In the publication of these volumes a very commendable service has been done for geography and for students in all lines making use of maps. For it is strictly true as the editor says in his preface, "atlases have not received the consideration in bibliography due to their importance in literature and as contributions to knowledge." There are few works on the subject and these are fragmentary.

The present contribution is merely a list of the geographical atlases in the Library of Congress, a total of over thirty-four hundred titles in addition to seventy lettered titles. The editor modestly disclaims it as a bibliography.

The arrangement is good. It starts with general atlases of special subjects, the subject headings in alphabetical order. Then follow the general atlases in chronological order, and then follow America, Europe, Asia, Africa and Oceanica in similar order. This classification includes under each general heading the atlases of cities, of voyages of circumnavigation, historical works, scientific explorations, and the atlas material accompanying the reports on boundary disputes between nations.

Bibliographical notes and tables of contents have been given in case of the rare and more important volumes. This brings out numerous inserted maps, so frequently hidden away in such material.

In the second volume the general index is preceded by an author index of abridged titles, in which the author's full name is

given, and dates of birth and death, where known. The importance of the latter is evident, as a clue to the dates of publication, for it has been the custom among most map publishers to omit the date. For obviously, since people as a rule are not very particular about maps, and know very little about them, it has always been a temptation to the publisher to make an old plate do in a new publication.

As no other library "has published a complete description of its atlas material it is impossible to state authoritatively how the collection in the Library of Congress compares in size and importance with others." But these two volumes certainly attest to long and assiduous collecting. To start at the beginning, of the forty known editions of Ptolemy, all but three are in this collection. In cartographic material relating to America the collection is especially rich and complete.

To all students in geography and history, these volumes will come as a welcome instrument of research. It will be of the highest value to be able to turn to the index for a place name, and to find there listed every atlas in the collection pertaining to the region, and in the more important publications to find even the description of every map in the atlas of the region. It will save endless search and will settle in a minute at your own desk, whether or not you have all the available material bearing on your particular quest.

Yet a hasty scanning of the collection seems to show a shortage of the most recent published material. And it raises the question, whether or not the appropriations for this division are generous enough to permit the acquisition of such fine atlas material as is available from the working presses of the day in the various lands. These two volumes at once will turn the attention of all the country to this collection, and it will be looked to whenever a map or atlas is desired. It is likely to raise uncomfortable questions when some of the best modern material from various lands is not found listed.

J. PAUL GOODE

UNIVERSITY OF CHICAGO,
May 23, 1910

SCIENTIFIC JOURNALS AND ARTICLES

THE April number (volume 11, number 2) of the *Transactions of the American Mathematical Society* contains the following papers:

Edward Kasner: "The theorem of Thomson and Tait and natural families of trajectories."

F. W. Owens: "The introduction of ideal elements and a new definition of projective n space."

Arthur Ranum: "The groups of congruent quadratic integers with respect to a composite ideal modulus."

G. D. Birkhoff: "A simplified treatment of the regular singular point."

L. M. Hoskins: "The strain of a gravitating, compressible elastic sphere."

THE May number (volume 16, number 8) of the *Bulletin of the American Mathematical Society* contains: Report of the February meeting of the Society, by F. N. Cole; Report of the February meeting of the San Francisco Section, by C. A. Noble; "An application of the notions of general analysis to a problem of the calculus of variations," by Oskar Bolza; "The infinitesimal contact transformations of mechanics," by Edward Kasner; "On an integral equation with an adjoined condition," by Anna J. Pell; "The unification of vectorial notations" (review of Burali-Forti and Marcolongo's Calcolo vettoriale and Omografie vettoriali), by E. B. Wilson; Shorter notice of Meyer's Allgemeine Formen- und Invariantentheorie, volume 1, Binäre Formen, by Virgil Snyder; "Notes"; "New Publications."

THE June number of the *Bulletin* contains: Report of the April meeting of the society, by F. N. Cole; Report of the April meeting of the Chicago Section, by H. E. Slaught; "Groups generated by two operators each of which is transformed into a power of itself by the square of the other," by G. A. Miller; "The solution of an integral equation occurring in the theory of radiation," by W. H. Jackson; Review of Grassmann's Projective Geometrie der Ebene, by L. W. Dowling; Review of Schlesinger's Lineare Differentialgleichungen, by E. J. Wilczynski; "Shorter notices": Bonola's

Geometria noneuclidea and Liebmann's German translation, by Arthur Ranum; Nichol's Analytic geometry, revised edition, and Wentworth and Smith's Complete arithmetic, by G. H. Scott; Wangerin's Theorie des Potentials und der Kugelfunktionen, by J. B. Shaw; Timerding's Geometrie der Kräfte, by W. R. Longley; "Notes"; "New Publications."

BOTANICAL NOTES

FORESTS AS GATHERERS OF NITROGEN

At a meeting of the Society of American Foresters, on March 31, 1910, a paper was read by Mr. Treadwell Cleveland, Jr., on "Forests as Gatherers of Nitrogen." This paper summarized results recently obtained by Jamieson, of Scotland, and by Zemplen and Roth, of the Royal Hungarian Experiment Station at Selmecbanya, which tend to show that forests are able to appropriate free atmospheric nitrogen by means of their trichomes. Jamieson investigated several forest trees (as well as a number of smaller plants), among which were *Acer campestre, Tilia europaea, Ulmus campestris, Sorbus aucuparia, Fagus silvatica* and *Picea concolor.* Zemplen and Roth included a large number of additional species. In all cases chemical tests show the presence of nitrogen in the trichomes, and the investigators believe that they have excluded all other sources for this nitrogen than the atmosphere. Professor Henry, of the Forest School at Nancy, France, was the first to point out that forest soils are enriched in nitrogen by the decay of fallen leaves.

Zemplen and Roth are cautious in their conclusions, and urge that further investigations be made in this field.

A STUDY OF PEAT-BOG FLORAS

In the last Report of the Iowa Geological Survey Professor L. H. Pammel discusses the peat flora found in the swamps and marshes of Iowa. For the bog formations he follows C. A. Davis's monograph. These bogs are not of the *Sphagnum* type usually associated with the term, but are listed by the author as follows: Quaking aspen bog, willow bog, grass and sedge marshes, rush marshes, moss bogs. The bog floras of Iowa, Wisconsin, southern Michigan and the Dismal Swamp Virginia are compared from a list of three hundred or more plants, showing strikingly the differences in their constitution.

The following observations may be noted. *Sphagnum, Larix laricina, Thuya occidentalis* and *Picea mariana* are not found in the state. Heaths are absent from the swamp flora. Of the fifteen plants listed by Transeau as characteristic of the bogs of northern America only five are found in the bogs of Iowa. Certain plants common to the peat bogs of regions farther north are not in the bogs of Iowa but are found in the colder and less fertile locations. *Carex filiformis* is the best peat former in the state.

The author discusses the important contributions to the subject, and gives a bibliography.

THE PRINCIPLE OF HOMOEOSIS

About a year ago Professor R. G. Leavitt published (*Bot. Gaz.*, January, 1909) a paper entitled "A Vegetative Mutant and the Principle of Homoeosis in Plants," which has not received the attention it deserves at the hands of botanists, no doubt partly due to the fact that it was not fully understood, and also that botanists, as a rule, are not greatly interested in underlying principles. They are so busy with the collection of solid facts of one kind and another that such "vague and insubstantial" things as principles have little attraction for them. This may account for the assertion made by a well-known professor of philosophy in a gathering of botanists a few years ago, namely, that "while botany has had many eminent men, it has been singularly unproductive in giving to the world any conspicuous general principles." Be this as it may, the fact remains that scant attention has been given to the paper here referred to, and to the principle which it sets forth.

Beginning with some familiar cases of leaf abscission, and of the decompounding of

other leaves, the author takes up the discussion of these and numerous related phenomena. He sees in them a trans-location of characters, that is, the transfer of characters from one structure to other structures, which latter may be further along in the ontogenetic line, or not so far along, or may belong to the alternative generation, or may be morphologically non-equivalent to the structures from which the transferred characters are borrowed. This transposition of characters he terms homoeosis, and in a paper of nearly forty pages illustrates and expands the principle with much force, and with convincing logic. Having established to his own satisfaction, at least, the doctrine of homoeosis, he is prepared to deduce certain conclusions as follows: "The study of homoeosis must somewhat increase the caution with which we use deviations from the normal as aids to morphological interpretation," a statement to which we fancy there will be little objection by any one, and which, it is to be hoped, will be taken to heart by morphologists and descriptive botanists the world over. It becomes evident that "relationship" may have a very different meaning when once we are aware of the facts of homoeosis, such as these which Professor Leavitt has so forcefully brought out in this paper. This service alone to morphology should justify the doctrine of homoeosis. His second conclusion that homoeosis has played some part in the evolution of plants will meet with little opposition. Lastly the author holds that the idea of homoeosis unites for descriptive purposes a great number of facts of ontogenesis which possess a considerable prospective value in connection with the effort to reach a correct mechanical interpretation of development.

CHARLES E. BESSEY
THE UNIVERSITY OF NEBRASKA

PALEOGEOGRAPHY OF NORTH AMERICA[1]

FEW articles of greater general interest have appeared in the *Bulletin of the Geological Society* in recent years than this. The paper may be divided into two parts—(*a*) an introductory portion dealing with methods, criteria and principles of paleogeography, and (*b*) the sequence of events in North America.

The author emphasizes the paleontologic method as of first importance. The distribution of seas is to be inferred from the distribution of faunas. The faunas are kept apart by barriers, of which the most important are land barriers. The local effect of currents in which there are differences of salinity or temperature is recognized, but the author thinks this can not be appealed to as an explanation of most faunal differences of the past. The physiographic testimony furnished by the sediments themselves is recognized as having a modicum of value, which in some kinds of deposits rises to first importance; but in general the usefulness of such data is not regarded as large. The important diastrophic events of geologic history are used to divide the course of time into eras and periods, and it is also pointed out that minor oscillations are often responsible for individual formations.

Following the views of Suess, Willis and others, Schuchert regards the continent as a mosaic of positive and negative elements; that is to say, regions which have shown a tendency to stand out of water as against regions which have been subject to repeated submergences. The location and general outline of these elements as conceived by the author are represented on two maps.

The commendable caution of Suess is followed in speaking of geographic changes not as uplifts and subsidences, but as "positive and negative displacements of the strand line," or as emergences and transgressions. The emergences are ascribed to periodic subsidence of the ocean bottom, causing the epicontinental seas to be drawn off into the ocean basins. The transgressions, or advances, of the sea, are thought to be due to one or more of several causes: (*a*) the attraction of the sea by bold shore mountains, (*b*) the down warping of the continent into geosynclines, thus forming long trough-like seas, (*c*) the displacement of the sea level by the filling up

[1] Charles Schuchert, *Bull. Geol. Soc. Am.*, Vol. XX., pp. 427–606, Pls. 46–101, 1910.

of the sea bottom as a necessary complement of erosion, and (*d*) the settling back of the continents in relaxation after periods of folding.

Following this discussion of principles the author gives a list of the chief strand line displacements with interesting although avowedly crude estimates as to the percentage of the continental plateau submerged at each stage. A graphic presentation of the same conclusions is given in the form of curves on plate 101 at the end of the paper. Barrell contributes a theoretical inquiry as to the effect which radial shortening would have on the rate of the earth's rotation, and on the degree to which a given increase in that rate would cause a heaping of the oceanic waters in the equatorial regions during times of orogenic activity. He finds reason to think that there would be a bulging of nearly 100 feet for each mile of radial shortening, which would tend to draw down the waters in the polar and temperate regions, to keep it stationary in latitude 35° and to cause a rise of the sea level in the tropics.

The second and much larger part of the paper contains a systematic account of the distribution and migrations of faunas, the geographic changes, and in some measure the nature of the climate and topography during the periods from the Cambrian to the Tertiary. This is illustrated by fifty maps showing the author's interpretation of the geography at each of many stages. To give a summary of this part would not be possible in a review, as it would almost necessitate a rehearsing of the original paper, which is itself much condensed. The most important general fact to be noted is the radical rearrangement of the geologic time table, which is given, in comparison with the current classification, as follows.

From this brief description it is plain that Mr. Schuchert's paper is one of first importance to the student of historical geology. It will be most highly valued as an up-to-date synopsis of the sequence of strata with their contained faunas from the base of the Paleozoic to the Tertiary period, and it will serve as a hand-book of information for many a stratigrapher whose opportunities and experience have necessarily been less extensive than the author's.

THE NEW GEOLOGIC TIME TABLE

Old Classification.		*New Classification.*	
Paleozoic	Cambrian	Georgic, Acadic, Ozarkic or Cambric	Paleozoic.
	Ordovician or Lower Silurian	Canadic, Ordovicic, Cincinnatic	
	Siluric	Siluric	Neopaleozoic.
	Devonic	Devonic	
	Mississippian or Sub-Carboniferous	Mississippic, Tennesseic	
	Pennsylvanic	Pennsylvanic	
	Permic	Permic	
Mesozoic	Triassic, Jurassic	Triassic-Jurassic	Mesozoic.
	Cretaceous	Comanchic, Cretacic	
Tertiary or Cenozoic	Eocene, Oligocene	Eogenic	Neozoic.
	Miocene, Pliocene, Pleistocene	Neogenic	

Any one who has attempted the construction of paleogeographic maps knows the uncertainties with which the work is beset and the impossibility sometimes of knowing just where a particular shore line should be drawn. Under these circumstances it requires courage to put one's many doubtful views in the unchangeable record form of a map, and Mr. Schuchert is to be commended for what he has done in this way and for his interesting table of strand line displacements mentioned above. The imperfection of these is distinctly recognized in the author's introductory remarks and the aid of other students of the subject is solicited by him in making the maps agree with the progress of future discoveries. Doubtless many readers of the paper will, like the reviewer, be disposed to take issue with Mr. Schuchert on many matters of detail, but these are hardly within the province of a review.

The radical changes in the geologic time scale will probably arouse more differences of opinion than any other single feature of the article. It may be asked first whether each of

these changes is justified, and in the second place, whether they are likely to be accepted. Like other innovations, these will have to be tried out by the test of time and usage. It may be suggested in this connection that, if the Cambrian and Ordovician are to be bracketed as an era, the Pennsylvanian and Permian should also be set off by themselves for reasons which are well brought out by Mr. Schuchert's own discussion of these periods. To the reviewer it appears even more just that the Mesozoic era should be divided into two, the line of separation being marked by the intense and widespread Sierran disturbance. To be consistent in having periods based on diastrophic movements, the author should also combine the late Devonian with the Mississippian as one period,—a procedure which is sanctioned in effect on page 493, where it is said " . . . the diastrophism at the conclusion of the Devonic does not appear to have been marked in character. . . . In this instance the life record is thought to have greater value than the physical one in separating the Devonic from the Mississippic, but should the principle of diastrophism be the sole guide, then these two periods seemingly must be combined into one."

A study of the paper brings out the fact that the author has worked largely from the point of view of the paleontologist, excluding in large measure the data of other sides of geology. Indeed, this may be inferred directly from the author's own paragraphs on methods. On page 525 it is remarked that "these maps . . . are still inadequate, as far as presenting a final . . . geographic distribution of the various faunas is concerned." In other words the maps are really faunal maps rather than strictly geographic. That is to say, they show the distribution of fossils rather than of land and sea. Perhaps the author will contend that these are one and the same, but it is quite certain that others will dissent from this view and with much to be said on their side. In the reviewer's judgment, valuable information can be drawn from certain sources of which Mr. Schuchert appears to have availed himself only in small measure, namely, the character and changes in the structure and composition of the sediments and the relations of conformity and unconformity between them. For example, the author excludes the interior sea from the Utah-Montana region at various times in the Paleozoic era, because the necessary faunas have not been found; in the face, however, of the fact that in many places an unbroken sequence of marine deposits has been found ranging from middle Cambrian to Mississippian. Many stratigraphers will not agree that the failure to find a fauna in a given section proves the existence of a "break" or "stratigraphic hiatus," much less a "disconformity." If the section is completely exposed and *if there is no physical evidence of an unconformity* it would seem that the burden of proof rests upon any one who doubts that sedimentation was continuous during the periods in question, whether or not the faunas are present.

A reading of the paper gives the impression that the author recognizes only two important factors which cause differences in faunas, *i. e.*, time and geographic isolation; in other words, that the Cambrian and Ordovician faunas of New York are unlike because one is much later than the other, while the Cambrian faunas of New York and Utah are dissimilar because they lived in marine provinces between which migration was impossible. It is occasionally admitted in the paper, however, and is generally recognized by biologists, that a third factor is operative—the environmental or edaphic factor. That the author is aware of this is indicated by the statement on page 589: "The wide difference between the Cretacic of Mexico and that of the United States may be due in part to the decided limestone facies of the former region. . . ." But in most other instances where this factor might well come into play it seems to have been left out of consideration. Thus on page 550 it is remarked that the "wonderful Burlington crinoid fauna" is unknown "in the western sea." Since crinoids prefer certain environmental conditions and have by no means a uniform distribution on the modern

sea bottom, may not the edaphic factor help to explain the observed distribution, particularly since the dark Mississippian limestones of Utah and Montana are notably unlike the contemporaneous rocks of Iowa?

The lack of evidence on debatable points throughout the paper is a constant source of disappointment to the reader. Thus on page 454 it would be interesting to know what leads the author to suggest central California as the site of an inlet from the Pacific Ocean rather than some other part of the coast. The Paleozoic rocks are so highly metamorphosed or so deeply buried from Mexico to Alaska that only here and there (as in northern California and Oregon) are they clearly recognizable, and to the average geologist there seems to be no ground for choosing any particular spot for the purpose indicated. This deficiency is probably one which the author could not easily prevent. It is to be remembered that the subject is over-large to cover adequately in so brief a space. It may be hoped that Mr. Schuchert will soon find time to prepare a volume or volumes under the same heading, in which he will give the desired facts which support his views.

Two things will tend to detract from the confidence with which this important and otherwise impressive paper will be received by geologists in general. One is the nonchalant way in which questions of a complex nature are dismissed as if they were matters of established belief. For example, on page 490 one finds the implication that the origin of dolomite is a matter of common knowledge whereas it is still an unsolved riddle to keen students of the subject. Again on page 447 is the statement, "Oolites are formed in the littoral region of seas between tides. . . ." This may explain some oolites, but several other explanations have been offered and it can not be truthfully said that the subject of the origin of oolites is yet understood.

The second and more serious defect is the assertive and dogmatic form in which many a debatable matter is presented. Examples of this are abundant throughout the paper, but the following will illustrate: (page 453) "Its syncline (Rocky Mountain sea) was due to thrusting of the Pacific mass. . . ." There is still much difference of opinion among the best students of the subject as to just what causes the warping of land surfaces. (Page 459) "Throughout the Paleozoic the northern Atlantic waters were separated from the southern Atlantic by the great continent Gondwana, uniting Africa and South America across the medial region of the present Atlantic. It is, therefore, not correct to speak of the northern Atlantic until the present form of this ocean has been attained. . . ." The existence of the Afro-American land bridge, although indicated by a considerable mass of evidence, is denied by many whose opinions are worth considering. (Page 495) "There was no Cordilleran sea of this time" (late Mississippian). In this case the unequivocal assertion of the author can be as positively refuted since a rich Kaskaskia fauna was discovered last year in the Wasatch Mountains of Utah.

In conclusion, and after offering these criticisms, the reviewer desires to repeat that the paper is a storehouse of information and a large contribution to the subject—the fruit of many years of careful study by a man well qualified as a paleontologist and blessed with unusual opportunities in the way of facilities and associations. Even so soon after its appearance it is plain that the paper is stimulating interest in the relatively new and still plastic science of paleogeography, in which much must be accomplished before firm foundations can be reached.

Eliot Blackwelder
University of Wisconsin,
April 25, 1910

SPECIAL ARTICLES

Webber's "brown fungus" of the citrus whitefly (Ægerita webberi n. sp.)

H. J. Webber discovered this fungus in 1896 growing parasitically upon the citrus whitefly at Manatee, Fla. He described in detail the sterile form of the fungus.[1] This

[1] U. S. Dept. of Agr., Div. of Veg. Phys. and Path., Bul. 13, 27–30, 1897.

fungus when it first develops on the under side of an orange leaf in larvæ of the white-fly, forms a chocolate-brown (Saccardo's color chart, No. 10) stroma, which somewhat resembles the citrus red scale, *Chrysomphalus aonidum*. From the margins of this stroma there extend colorless thick-walled hyphae. This stage of the fungus is sterile, and in this condition it was described by Webber under the name of "Brown fungus."

In the later development of the fungus (usually in the summer or fall) it sends out long, straight, colorless hyphæ, which grow, not only over the under surface of the leaf, but around the edges and upon the upper surface. On the upper surface of the leaves, upon short lateral branches of these hyphæ, are borne aggregations of cells, which seem to be characteristic sporodochia of the genus-form *Ægerita*. These sporodochia are 60 to 90 microns in diameter, and are more or less spherical clusters of inflated oval cells, 12 to 18 microns in diameter. From near the place of attachment of the sporodochium there radiate 3 to 5 hypha-like appendages, 150 to 200 microns long by 6 to 8 microns wide, one to three septate. This entire aggregation of spherical cells and appendages remains in unison, and functions as a spore. When abundant, these sporodochia present to the eye the appearance of a reddish-brown dust over the upper surface of the leaves. If the lower side of a leaf bearing brown fungous stromata happens to be turned upward for some time, the sporodochia will develop abundantly there. These sporodochia were first noticed in the fall of 1905, accompanying the "brown fungus"; but only recently has the connection between the two been proved. Their supposed connection was touched upon in 1908.[2]

These sporodochia are curious and interesting. When once detached from the leaf, they blow about on smooth surfaces at the least motion of the air, but on alighting upon another leaf or fairly rough paper, they tend to hold fast to it.

[2] "Fungi Parasitic upon *Aleyrodes citri*," Univ. of Fla., Special Studies, No. 1, p. 36.

When germinated in hanging-drop cultures these sporodochia produce hyphæ identical with those of Webber's "brown fungus." When the sporodochia are placed upon the larvæ of *Aleyrodes citri*, typical stromata of the "brown fungus" arise. During the summer and fall of 1909, sporodochia were carefully picked off under a compound microscope. A camel's hair brush, moistened with water containing these sporodochia, was drawn over live whitefly larvæ. Nine days after, the first and second stage larvæ began to show the effects of fungous infection. In sixteen days, initial stages of the stromata were evident bursting through the edges of the larvæ. At a later date, the typical brown stromata were formed, and in three months *Ægerita* sporodochia were produced by the surface hyphæ on the upper sides of the leaves.

The economic importance of this fungus makes it desirable that it should have a scientific name. The form of the sporodochium most nearly resembles that of the provisional genus *Ægerita*. The fungus was referred to Dr. Roland Thaxter, of Harvard University, who kindly examined it, and confirmed the view that it might well be placed under the name of *Ægerita* until the perfect stage was found. It is therefore proposed to designate Webber's 'brown fungus" as *Ægerita webberi* n. sp. The form and appearance of the hyphæ suggest relationships to the Hypochnaceæ of the basidiomycetous fungi.

H. S. FAWCETT

A CORRECTED CLASSIFICATION OF THE EDENTATES

IN a recent paper[1] the writer was led, from a consideration of various anatomical characters, to the recognition of the Edentata as a *superorder* of mammals comprising four distinct *orders*, as follows:

SUPERORDER EDENTATA (Vicq d'Azyr).

Order 1. TÆNIODONTA Cope.

Order 2. XENARTHRA Gill.

[1] "A Suggested Classification of Edentates," State University of Oklahoma, Research Bulletin, No. 2, 1909.

Order 3. Pholidota Weber.
Order 4. Tubulidentata Flower.

A further examination of the literature reveals the fact that the term Pholidota Weber (1904), comprising the *Manidæ*, is antedated by Pholidota Merrem ("Tentamen systematis Amphibiorum," 1820), applied to the *Reptilia*. As Squamata Huxley (1872), which also has been frequently used to designate the *Manidæ*, is itself antedated by Squamata Oppel (1811), applied to an order or superorder (Osborn) of Reptilia, it seems necessary to adopt some other name for this group. I therefore propose that the order to which the Manidæ belong, be called the Lepidota [Gr. λεπιδωτός, scaly].

Making this change and listing the families, our classification of the Edentates is as follows:

SUPERORDER EDENTATA Vicq d'Azyr.
Order 1. Tæniodonta Cope.
Family *Conoryctidæ* Wortman.
Family *Stylinodontidæ* Marsh.
Order 2. Xenarthra Gill.
Suborder Pilosa Flower.
Family *Bradypodidæ* Bonaparte.
Family *Megalonychidæ* Zittel.
Family *Megatheriidæ* Owen.
Family *Myrmecophagidæ* Bonaparte.
Family *Orophodontidæ* Ameghino.
Suborder Loricata Flower.
Family *Dasypodidæ* Bonaparte.
Family *Glyptodontidæ* Burmeister.
Order 3. Lepidota Lane.
Family *Manidæ* Gray.
Order 4. Tubulidentata Huxley.
Family *Orycteropodidæ* Bonaparte.

H. H. Lane
State University of Oklahoma,
Norman, Oklahoma,
February 15, 1910

THE NORTH CAROLINA ACADEMY OF SCIENCE

The ninth annual meeting of the North Carolina Academy of Science was held at Wake Forest College, Wake Forest, N. C., on April 29 and 30, 1910, with thirty-one members in attendance. The meeting of the executive committee, held on the afternoon of April 29, was followed by a general meeting for the reading and discussing of papers. At night in Wingate Memorial Hall, the academy was formally welcomed to Wake Forest College by President W. L. Poteat. President W. C. Coker, of the academy, then delivered the presidential address, "Science Teaching in the Schools and Colleges of North Carolina."

Because of their interest to the general public, the following papers were then given with lantern slide illustrations and diagrams: "Pellagra," a preliminary report, by Professor J. J. Wolfe, of Trinity College; "Halley's Comet," by Professor A. H. Patterson, of the University of North Carolina; "The Comet, What is It?" by Professor John F. Lanneau, of Wake Forest College.

On Saturday morning, April 30, the academy reconvened for the annual business meeting. The reports of the secretary-treasurer and of various committees were heard. Forty-six new members were received into the academy. These, together with the 43 former members, give a total membership of 89. The report of the treasurer showed the finances of the academy to be in a very flourishing condition. A large and representative committee was appointed to collect data and report to the next meeting of the academy a course of study in the sciences for the high schools of the state. It is the purpose of the academy to transmit this with its recommendation to the state superintendent of public instruction and to the North Carolina Teachers' Assembly.

The following officers were chosen for the ensuing year:

President—W. H. Pegram, Trinity College, Durham, N. C.

Vice-president—W. S. Rankin, State Board of Health, Raleigh, N. C.

Secretary-Treasurer—E. W. Gudger, State Normal College, Greensboro, N. C.

Executive Committee—F. L. Stevens, A. & M. College, W. Raleigh, N. C.; H. H. Brimley, State Museum, Raleigh, N. C.; H. V. Wilson, University of North Carolina, Chapel Hill, N. C.

In point of attendance, number of new members added, number of papers read, general interest as shown in the discussion of papers, this meeting excelled any since the founding of the academy.

The following papers were presented:

The Cause of Pellagra (a preliminary report): Jas. J. Wolfe, Trinity College, Durham, N. C.

Believing that pellagra must be an infectious disease, and that, because of its generalized nature, the organism was most likely to occur in the blood, the writer, last September, began a

study of some specimens of pellagrous blood with the hope of throwing some light on the etiology of this disease.

The usual smear preparation was made, stained with methylene blue and studied under a Zeiss apochromat. Bacteria were seen in considerable numbers in most cases—especially severe ones. Milder cases were more difficult and not as yet entirely convincing. These bacteria are polymorphic, but generally spherical, grouped often in doubles like a dumb-bell or in irregular clumps, sometimes in chains and usually between .5 and 1 μ in diameter.

A culture derived from damaged corn shows an organism quite similar in grouping, size, color reactions and polymorphism. This is now being tested with animals.

Peculiarities in Distribution of North Carolina Birds: FRANKLIN SHERMAN, Jr., Raleigh, N. C.

The main points brought out in this paper are as follows:

1. The song sparrow was long known to breed mainly if not exclusively on the very verge of the coast region. Records were given showing that it nests quite freely in the mountain region also. There is no evidence that it nests in any of the central sections of the state.

2. The towhee has been known to breed only in the eastern and western sections. Data were given showing that it also nests in the central section to some extent, though perhaps not so abundantly.

3. The barn swallow has been known to nest only on the coast. A record was given of its nesting at about 2,600 feet elevation in the mountains. It is not known in nesting season in the central part of the state.

4. The loggerhead shrike is mainly a winter visitor, going north to breed. Two or three breeding records are on file. Several new records are added, especially from the eastern section.

5. The robin has been known to breed only in the western half of the state. Data were given showing that in 1909, at least, it nested in a number of eastern localities. It may be extending its breeding range to the southward.

The tendency shown by certain birds (confirmed by some other animals and plants) to occur in the eastern and western extremes of the state is attributed to high humidity of the coast region which gives to the plants or animals the conditions of a more northern latitude. The western part of the state furnishes the same conditions by altitude.

The Comet: What is It? JOHN F. LANNEAU, Wake Forest College, Wake Forest, N. C.

The Resin of Pinus sabiniana: CHARLES H. HERTY and E. N. TILLETT, University of North Carolina, Chapel Hill, N. C. (Read by title.)

Medical Entomology: Z. P. METCALF, Department of Agriculture, Raleigh, N. C.

A short popular account of some of the more recent developments in the science of medical entomology, which was defined as that branch of entomology which treats of the relation of insects and insect-like animals in the transfer of diseases from man to man, man to animal and animal to animal. This relation was declared to be twofold: In the first case the insect is a necessary intermediate host and in the second case the insect is merely an incidental or accidental factor in the transfer of the disease. The work of the board of health of the city of Asheville, N. C., was cited as an example of applied medical entomology.

The Ammonifying of North Carolina Soils: F. L. STEVENS and W. A. WITHERS, assisted by P. L. GAINEY and F. W. SHERWOOD, North Carolina Agricultural Experiment Station, W. Raleigh, N. C.

Remarks on the Relation of our Birds to the Farm and Garden: C. S. BRIMLEY, Raleigh, N. C. (Read by F. Sherman, Jr.) Published in full in the current number of the *Journal of the Elisha Mitchell Scientific Society.*

Where to find Amebas: E. W. GUDGER, State Normal College, Greensboro, N. C.

The directions given in the books are very indefinite, as the writer found to his sorrow in his early biological days. Acting on a suggestion made by Dr. D. H. Tennent, now of Bryn Mawr College, he at that time successfully sought them in the yellowish-green diatom deposits on the bottom of stagnant ditches or of quiet pools in brooks. In seven years these have never failed to furnish abundant material. The writer's classes are supplied from a tiled drain at the foot of a bank less than one hundred yards from the laboratory. These amebas vary in size from quite small to those so large that they can not be seen in their entirety under the ordinary high objective.

The Origin of Thermal Waters, with Special Reference to Hot Springs, Ark.: COLLIER COBB, University of North Carolina, Chapel Hill, N. C.

Some Aids to Better Work in Science: C. W. Edwards, Trinity College, Durham, N. C. (Read by title.)

A new Hybrid Habenaria of North Carolina: J. G. Hall, North Carolina Agricultural Experiment Station, West Raleigh, N. C.

A hybrid *Habenaria* was reported from the neighborhood of Kinston, N. C. This natural hybrid seemed to be pretty well intermediate between the two supposed parents *H. ciliaris* and *H. blephariglottis*. Photographs of the flowers were shown and these presented some characters of the parents and the hybrid.

The Present Status of the Darwinian Hypothesis: W. L. Poteat, Wake Forest College, Wake Forest, N. C.

Some Experiments on Ionization by Impact: The Time Variation of a Current through a Gas Ionized by Radium: J. Blanchard, Trinity College, Durham, N. C.

The ionization vessel was a glass tube with parallel plate electrodes about five centimeters in diameter, both plates coated (though unequally) with a thin layer of a very impure salt of radium. With the plates about one centimeter apart, and the pressure about one millimeter, with a potential difference sufficient to produce considerable ionization by impact, it was found that the current decreased with the time the battery key remained closed, reaching its minimum value in about an hour. On opening the key the initial conductivity was almost totally regained in about the same time. Upon reversing the potential at the end of an hour the current was sometimes found to be greater than it was initially in this reverse direction, but also decreasing with the time as before.

The potential difference apparently causes an increased amount of ionization near the positive plate.

Further experiments are in progress.

Is the Fusarium which Causes Cowpea Wilt Genetically connected with Neocosmospora? B. B. Higgins, North Carolina Agricultural Experiment Station, West Raleigh, N. C.

In 1889 the wilt disease of cotton was studied by Professor Geo. F. Atkinson and its causal fungus named *Fusarium vasinfectum*. A few years later (1894–99) the wilt disease of cotton, watermelon and cowpea was studied by Erwin F. Smith. He found no specific differences between the fungi upon any of the three hosts. He found, however, upon some of the plants previously killed by the wilt fungus, an acigerous fungus which he considered the perfect stage of *Fusarium vasinfectum*. The fungus was therefore renamed by him *Neocosmospora vasinfecta*, and this conclusion has been accepted by subsequent writers. The evidence upon which this conclusion was based was very weak, however; and a recent study of the two forms by the writer—the results of which will at an early date be published in bulletin form—has caused the writer to reopen this question which was considered closed.

Some Experiments in the Propagation of the Diamond-back Terrapin: Henry D. Aller, Fisheries Laboratory, Beaufort, N. C. (Read by the secretary.)

This paper appears in full in the current number of the *Journal of the Elisha Mitchell Scientific Society*.

The Present Status of the Relativity Problem: C. W. Edwards, Trinity College, Durham, N. C. (Read by title.)

The Locus of a Moving Point when the Sum of its Distances from Two Fixed Points, their Difference, their Product or their Quotient is Constant: John F. Lanneau.

The loci determined by the first three conditions are the well-known ellipse, hyberbola and lemniscate.

Under the fourth condition: Take line through the fixed points F and F' as x-axis; the point O, midway between them, as origin; $2c$ for distance F to F'; K for the constant quotient when the moving point is on one side of the y-axis, and therefore $1/K$ the quotient when it has the corresponding position on the other side.

1. The equation of the locus is

$$x^2 + y^2 \mp 2c\frac{K^2+1}{K^2-1}x + c^2 = 0.$$

The locus, therefore, consists of two equal circles whose centers are on the x-axis beyond F and F', at equal distances from O.

2. A discussion of the equation shows:

If $K = 1$, the circles are of infinite radius, and are tangent at O.

If K is either 0 or ∞, the circles reduce to the points F and F'.

If K has, in turn, any series of values between 1 and 0, or between 1 and ∞, the loci form a group of circles about F and a similar group about F'—the number of circles in each group limited only by the number of values given to K.

3. None of the circles of the F and F' groups pass through either of the fixed points F and F'.

Any circles drawn through F and F' are extraneous to the loci, but each such circle is orthogonal to every circle in the loci groups.

Notes on Fungi: F. L. STEVENS and J. G. HALL, North Carolina Agricultural Experiment Station, West Raleigh, N. C.

Three new species of *Claviceps* were described. Two of them are upon *Paspalum* and are thought to be the perfect stages of the fungus usually known as *Sclerotium Paspali* S. Germination of the sclerotium was described and the characters of the fungus were illustrated by photographs and specimens. The third species grows upon gama grass (*Tripsacum dactyloides* L.). Both sphacelia and ascosporic stages were exhibited. Technical descriptions were given under the names *Claviceps Paspali* (S.) n. comb.; *C. Rolfsii* n. sp., and *C. Tripsaci* n. sp. These will be published in full elsewhere soon.

Specimens of a *Cercospora* upon persimmon which was thought to be new were also shown.

Some Methods of Making Illustrations: Z. P. METCALF, Department of Agriculture, Raleigh, N. C.

A brief consideration of some of the more important methods of making illustrations considered from the standpoint of the biologist.

Precautions Necessary in Estimating Climates of Geological Time: COLLIER COBB, University of North Carolina, Chapel Hill, N. C.

The Jaws of the Spotted Sting Ray Aëtobatus narinari: E. W. GUDGER, State Normal College, Greensboro, N. C.

This ray and its jaws were described by George Marcgrave from a specimen from Brazilian waters in a book published in 1648. Unlike other pavement-toothed rays, this fish has only the central row of teeth, the lateral ones having entirely disappeared. Marcgrave correctly counted its fourteen I-shaped upper teeth, and its seventeen broad V-shaped lower ones. The lower jaw is narrower and longer than the upper and projects beyond the lips. With it and the snout the ray digs up the clams which constitute its chief food.

The paper was illustrated with photographs of the fish and with a pair of dried jaws.

The writer has in preparation for the U. S. Bureau of Fisheries, a paper on this ray, reviewing all the work ever done on it, and including his own observations and photographs.

The Cocoanut Crab: JOHN F. LANNEAU.

Called also the robber crab and the pouch crab. Shaped more like a lobster than a crab. Found on islands of the South Pacific. Weight usually five or six pounds, sometimes twenty. Feeds on fallen cocoanuts. Said to climb the trees. Is highly esteemed as food, especially the rich, fatty content of the pouch. Is found on our island of Guam. It and other singular forms of life on that pleasant little island would repay a biologist's investigation. His visit would likely be facilitated by our Secretary of War or Secretary of Navy.

A Double Flowering Dogwood: F. L. STEVENS and J. G. HALL, North Carolina Agricultural Experiment Station, West Raleigh, N. C.

A case of double flower of the common flowering dogwood (*Cornus florida* L.) due to the excessive development of the small bracts that subtend the individual flowers of the ordinary head was reported. There was as well the suppression of all the individual flowers except the central one, which appeared entirely normal.

A Note in the Development of the Gall-fly Diastrophus nebulosus O. S.: J. D. IVES, Wake Forest College, Wake Forest, N. C.

This paper is published in full in the current number of the *Journal of the Elisha Mitchell Scientific Society.*

Pecan Culture in North Carolina: W. N. HUTT, State Horticulturist, Raleigh, N. C.

E. W. GUDGER,
Secretary

SOCIETIES AND ACADEMIES

THE SOCIETY FOR EXPERIMENTAL BIOLOGY AND MEDICINE

THE thirty-ninth meeting was held at the Sheffield Biological Laboratory, New Haven, Conn., on Wednesday, May 18, 1910, at 4:15 P.M., with President Morgan in the chair. An executive meeting was held.

New members elected: A. B. Eisenbrey, H. D. Senior, Edna Steinhardt, H. F. Swift.

Members present: Atkinson, Beebe, Davenport, Gies, Harrison, Henderson, Janeway, Lee, Levin, I., Lusk, MacCallum, Meltzer, Mendel, Morgan, Murlin, Norris, Pearce, Shaklee, Stewart, H. A., Wolf.

Scientific Program

"An Examination of Fröhlich's Theory of the Treppe," Frederic S. Lee and E. N. Harvey.

"An Attempt to Discover the Cause of the Specific Dynamic Action of Protein," Graham Lusk.

"Demonstration of a Modified Method of Estimating Pepsin," William C. Rose. (By invitation.)

"The Metabolism of the Purines in Man," Lafayette B. Mendel and John F. Lyman.

"A Demonstration of the Method of Phelps and Tillotson for Esterifying the Products of Protein Hydrolysis," T. B. Osborne and L. M. Liddle.

"The Distribution of the Blood in Shock," E. P. Lyon and J. L. Swarts.

"The Fundamental Conditions of Surgical Shock," Yandell Henderson.

"Observations on the Nature of the Antitrypsin of the Serum," R. Weil and L. Feldstein.

"On the Power of Reproduction without Conjugation in Paramecium," Lorande Loss Woodruff.

"Alleged Rhythm in Phototaxis Synchronous with Ocean Tides," Max Withrow Morse.

"Vaso-response in Dogs to Hydrophobia Rabbit Serum," J. P. Atkinson and C. B. Fitzpatrick.

"On the Precipitation of Diphtheria Antitoxin by Precipitins," J. P. Atkinson and Edwin J. Banzhaf.

"Further Observations on the Structure of Anastomosed Blood Vessels," C. C. Guthrie.

"Results of Engrafting Fetuses into Fowls," C. C. Guthrie.

"Factors Influencing the Survival of Engrafted Thyroid Tissues in Fowls," C. C. Guthrie.

"Modification of Tissue Oxidations *in vitro*," F. V. Guthrie. (By invitation.)

"The Development and Function of the Heart in Embryos without Nerves," Davenport Hooker. (By invitation.)

"A Demonstration of the Use of Krogh's Gas Tonometer," M. M. Scarborough. (By invitation.)

"An Experimental Study of the Resistance to Compression of the Arterial Wall," T. C. Janeway and E. A. Park.

"A Device for Control of Ether and Air or other Gases in Connection with Various Forms of Artificial Respiration," A. O. Shaklee.

"The Chromosomes in the Parthenogenetic and Sexual Eggs of Phylloxerans and Aphids," T. H. Morgan.

"Hybridization in a Mutating Period in Drosophila," T. H. Morgan.

"Inflammation in Tissues Isolated from Nervous Connections," W. G. MacCallum.

"Experimental Hypertrophy of the Heart," H. A. Stewart.

"Biological Significance of Sertoli Cells," F. M. Hanes. (By invitation.)

"A Study of Saliva in its Possible Relation to Dental Caries," Alfred P. Lothrop and William J. Gies.

"Studies on Experimental Arterial Lesions in the Dog," Isaac Levin and John H. Larkin.

"The Relation of the Thalamus to Respiration, Blood Pressure and Blood Supply of the Spleen," E. Sachs. (By invitation.)

"The Influence of Oils and Lecithin on Protein Metabolism," Lloyd H. Mills and John R. Murlin.

"Inheritance of Plumage Color in Poultry," Charles B. Davenport.

Eugene L. Opie,
Secretary

THE NEW YORK ACADEMY OF SCIENCES
SECTION OF BIOLOGY

A regular meeting of this section was held at the American Museum of Natural History, April 11, 1910, Mr. Roy W. Miner presiding. The following papers were read:

Collecting Invertebrates in the Woods Hole Region: Roy W. Miner.

Mr. Miner gave an account of his collecting experiences during the summer of 1910 in the Woods Hole region. The methods and results of a dredging expedition were first outlined, and then the speaker gave an account of the habits of some of the more interesting and typical invertebrates found in the vicinity of Buzzard Bay and Vineyard Sound, dwelling especially on the Annulata. The address was illustrated with colored lantern slides of the living animals.

Osteology and Genetic Relations of the Menotyphlous Insectivores: W. K. Gregory. (Read by title.)

W. K. Gregory,
Secretary pro tem.

At the regular meeting of this section held at the American Museum of Natural History, May 9, 1910, Professor Bashford Dean presiding, the following papers were read:

Notes on the Insectivore Genus Tupaia and its Allies: W. K. Gregory.

In 1904 Dr. W. D. Mathew interpreted the characters of many Eocene mammals of various orders as pointing to a common stem form of arboreal habits and structure. The oriental insectivore *Tupaia*, and its little known Bornean ally *Ptilocercus lowii*, serve to illustrate these characters in still living forms. They have a divergent but not yet opposable thumb and great

toe, their habits are chiefly arboreal and the diet insectivorous-frugivorous. *Tupaia* retains many skeletal features that were characteristic of Eocene unguiculates, *e. g.*, long humerus and femur, humerus with entepicondylar foramen, femur with third trochanter, radius and ulna and tibia and fibula separate, flexible carpus and tarsus, semiplantigrade, five-toed manus and pes with divergent digit 1, free centrale carpi, astragalus without trochlear keels and with a rounded head, vertebral formula C. 7, D. 13, L. 6 or 7, S. 3, Cd. 23–26—and many others. Other features distinctly foreshadow the primate type, *e. g.*, relatively large brain case, broad forehead, large, posteriorly closed orbits, and especially the structural details of the auditory bulla and ossicles, dentition and astragalus. In *Ptilocercus* the skull and dentition is even more distinctly lemuroid but the rest of the skeleton is unknown.

It is of course possible that these lemuroid characters are entirely due to convergent evolution, but the provisional conclusion is that the Tupaiidæ are descended from the Insectivore stock that gave rise to the primates. Attention was called to the resemblances between *Ptilocercus* and the lower jaw from the Bridger Eocene described by Mathew as *Entomolestes grangeri*. The only differences are such as frequently separate more generalized forms from their descendants.

Fourth Journey of Exploration in the South Seas: H. E. CRAMPTON.

The speaker gave a brief account of the new results obtained in the course of a journey of seven months' duration among the Society, Cook, New Zealand, Tongan, Samoan, Fiji and Hawaiian islands. The organisms forming the material of investigations were terrestrial snails of the genus *Partula*—a strictly Pacific group. The species differ when a comparison is made of forms occurring in neighboring but isolated valleys of one island, in different islands of the same group, and in different groups of islands. The uniform principle of distribution summarizing the observed facts is, that the degree of geographic proximity of any two comparable regions is correlated with the degree of biological differentiation of their species.

A description was given of two active volcanoes, namely, of Savaii in Samoa and Kilauea in Hawaii. Other older islands of volcanic nature were brought into relation with these examples, as later stages in the production of deeply-furrowed land masses like Tahiti, where conditions are such that isolated valley stations are found to be the homes of separate colonies of snails. Regarding the relation of such islands to other weathered peaks like Borabora, to coral atolls and to islands of uplifted coral limestone like many examples in the Cook and Tonga groups, the Darwin-Dana doctrine was contrasted with the views of Agassiz. It was pointed out that the phenomena of distribution in the case of species of *Partula* gave unquestioned support to the Darwin-Dana doctrine of a major process of subsidence, although secondary sporadic examples of the reverse process of uplift may be demonstrated at different points of the South Pacific Ocean.

L. HUSSAKOF,
Secretary
AMERICAN MUSEUM OF NATURAL HISTORY

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 680th meeting was held on May 7, 1910, President Woodward in the chair. Three papers were read.

A Method of Precision for Computing Square Roots of Numbers: Dr. R. S. WOODWARD, of the Carnegie Institution of Washington.

This method depends on the identity

$$ab = \tfrac{1}{4}(a+b)^2\{1-(a-b/a+b)^2\}.$$

Let N be any positive number and write $N = ab$, wherein a and b are any two numbers whose product is N. Write also for brevity $x = (a-b/a+b)^2$.

Then

$$\sqrt{N} = \sqrt{(ab)} = \tfrac{1}{2}(a+b)(1-x)^{\frac{1}{2}}$$
$$= \tfrac{1}{2}(a+b)(1-\tfrac{1}{2}x-\tfrac{1}{8}x^2-\cdots).$$

It is seen that if the numbers a and b are properly chosen the series in x will converge very rapidly. They may be so chosen in fact that a high order of precision will be attained from the expression

$$\tfrac{1}{2}(a+b)(1-\tfrac{1}{2}x).$$

It is seen also that the calculation by means of the latter formula will be simplified if a and b are so chosen that $(a-b)/(a+b)$ is the reciprocal of an integer n, or if $x^{-1} = n^2$. This applies especially in case N is one of the natural numbers 2, 3, 5, ... When n is an integer the following relations hold:

$$a^2 = N \cdot n + 1/n - 1, \quad b^2 = N \cdot n - 1/n + 1.$$

When the approximation is limited to the first term in x, the exact value of the remainder, or error of the calculation, is

$$-\tfrac{1}{2}(a+b)\{1-\tfrac{1}{2}x-(1-x)^{\frac{1}{2}}\},$$

and this is the same as

$$-\tfrac{1}{2}(a+b)\,(\tfrac{1}{8}x^2+\tfrac{1}{16}x^3+\cdots).$$

The application of the process to the numbers 2, 3, 5 is illustrated by the following table of values:

N	a	b	$\frac{1}{2}(a+b)$	x^{-1}
2	$\frac{20}{14}$	$\frac{14}{10}$	$\frac{22}{70}$	$(99)^2$
3	$\frac{7}{5}$	$\frac{12}{7}$	$\frac{97}{56}$	$(97)^2$
5	$\frac{9}{4}$	$\frac{20}{9}$	$\frac{161}{72}$	$(161)^2$

The simplicity and precision of the calculations required are shown by the figures given below for the number 2.

$$99/70 = 1.4142857143,$$
$$-1/2\cdot 1/70\cdot 1/99 = -0.0000721501,$$
$$\text{Sum} = \sqrt{2} = 1.4142135642,$$
$$\text{Error} = - \qquad 18.$$

Halley's and other Comets: Professor Asaph Hall, of the U. S. Naval Observatory.

The speaker gave some interesting points about Halley's comet, including the date of its reappearance, its physical appearance, its relative brightness and orbital elements. This comet has been surely identified back to the year 1066 and probably to a much earlier date. The supposed physical constitution of comets was discussed at some length. Among the other comets mentioned were Brook's, Swift's and Encke's. The orbits of most comets are parabolic, or nearly so. A number of planets have their own family of comets. Comet captures by planets and the perturbation effects of the sun and the planets were briefly discussed.

Is there an Emanation from a Magnetized Substance? L. A. Bauer, of the Carnegie Institution of Wasnington.

The purpose of the paper was mainly to direct attention to the fundamental assumptions which underlie our explanations of magnetic phenomena. The question was raised as to what evidences there may be for or against the hypothesis of a possible "emanation"—using that word in its most general sense, radiation, pulsation or emission—due to the presence of a magnetized substance so that the force exerted by the latter might, like electric force, be corpuscular in its nature. The corpuscles in magnetism might be atomic systems in which an electron is revolving about an inner nucleus consisting, for example, of a positive ion, such as assumed by Righi for the formation of his so-called "magnetic rays." Righi calls his atomic system an electron-positive ion, and Thomson, who independently of Righi had occasion to consider the possibility of similar systems, termed them "doublets." Since the system creates an atomic magnetic field whose axis passes through the center of rotation of the electron and perpendicular to the plane of rotation the speaker suggested calling such systems "magnetons." These magnetons, carrying a free magnetic charge, if given a translational movement along the magnetic axis, will possess all the properties ascribed to the lines of magnetic force—the translational movement giving the tension along the lines of force and the centripetal acceleration of the revolving electron supplying the cross pressure.

Some results obtained by the speaker in connection with his careful weighings, in a wholly non-magnetic balance, of magnetized and unmagnetized substances, led him to consider the hypothesis of a mechanical force being exerted on a magnet by the outside medium due to a possible emanation or pulsation of some kind from the magnet. Further experiments are to be made.

If the hypothesis as above set forth is correct we may look upon a magnetized substance as a source of "magneto-activity" just as a radioactive substance is of radioactivity.

(The abstracts of the first and third of the above mentioned papers are by their authors.)

R. L. Faris,
Secretary

THE AMERICAN CHEMICAL SOCIETY
NORTHEASTERN SECTION

The ninety-eighth regular meeting of the section was held at the Twentieth Century Club, Boston, on April 29. Professor Henry Carmichael presented a paper entitled "Electrolysis of Chlorides Theoretically Considered," in which he described the advantages of a partition of asbestos cloth impregnated with portland cement, as a means of separating the electrode products in the analysis of brine. Mr. Jasper Whiting, in a paper upon "The Electrolysis of Brine," described in detail his electrolytic cell which makes use of the formation of sodium amalgam but is not open to many disadvantages possessed by the "Castner process."

K. L. Mark,
Secretary